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THE

AMERICAN JOURNAL

OF THE MEDICAL SCIENCES

FOR JANUARY 1871.

ART. I.—*On Irritable Heart; a Clinical Study of a Form of Functional Cardiac Disorder and its Consequences.* By J. M. DA COSTA, M. D., one of the Physicians to the Pennsylvania Hospital, etc.

IN this paper I propose to consider a form of cardiac malady common among soldiers, but the study of which is equally interesting to the civil practitioner, on account of its intimate bearing on some obscure or doubtful points of pathology. Much of what I am about to say I could duplicate from the experience of private practice; yet I prefer to let this inquiry remain as it was originally conducted on soldiers during our late war. The observations here collected were made on a series of upwards of three hundred cases. That so large a number were examined is thus explained. Shortly after the establishment of military hospitals in our large cities, I was appointed visiting physician to one in Philadelphia, and there I noticed cases of a peculiar form of functional disorder of the heart, to which I gave the name of irritable heart—a name by which the disorder soon became known both within and without the walls of the hospital. In a communication addressed to the Department in December, 1862, I further called attention to this form of cardiac malady, more particularly as it was observed subsequent to the Peninsular campaign. Afterwards, through the fostering care of the Medical Directors and Inspectors of this Military District, and the liberality and kindly interest of the Department at Washington, most of the cases of the kind were sent to my wards, thus enabling me to study the affection on a large scale. Some of

the general facts recognized by this investigation I published in April, 1864, in the first edition of my *Medical Diagnosis;* but it was a mere outline, and the inquiry being pursued further, I intend here to give the complete results. The publication of the paper so long after the observations were made has been delayed by several causes; partly by want of leisure to analyze critically so large a number of cases; partly because it was my original intention to have offered this contribution as a report to the official history of the war; but chiefly because I found that, as I still from time to time encountered my former patients, I should by waiting have the opportunity of ascertaining the sequel to many of the cases recorded. And this—for reasons which will soon become evident—struck me in the examination of the subject as of particular value.

The class of cases which I am about to analyze was encountered in every army of the United States, and attracted the attention of many of its medical officers. Yet it cannot be said that ours was the first war in which it was noticed; for we find in the British Blue Book of the Crimean war, in the Report of the Hospitals of the Army in the East, as separate from morbus cordis, or carditis, pericarditis, etc., forty-five admissions for palpitation, and in another table, giving the return "invalided to England," sixty-two cases are thus classed; and I have seen here and there short statements which make it likely that the same affection was noticed in India, among Sir Henry Havelock's gallant troops. Nor can I believe that it has not always existed. Looking at the causes which produce it, it must have occurred among the troops in Sir John Moore's retreat, where, as Napier tells us, some brigades covered the retreat, fighting for twelve days, and traversed eighty miles of road in two marches; or among the devoted band by whose energetic movements Lord Clive conquered India; or among the victorious columns which, under Napoleon's guidance, passed by forced marches from the Rhine to the Danube, and compelled, at Ulm, the surrender of an army; or among the shattered and harassed forces which found their way back from the disastrous invasion of Russia. Yet, in examining, so far as I could obtain them, the documents bearing on these movements, I do not find the subject mentioned. But the disorder not being recognized, is no proof of its not having existed; for, when we reflect how almost entirely the accurate knowledge of diseases of the heart is the knowledge of our times, we can readily understand how difficult or impossible it may have been to have distinguished the less marked groups.

That the affection about to be described is not confined to troops engaged in actual warfare, may be judged by a statement of Coche,[1] that soldiers kept long under drill, are liable to functional derangement of the heart with palpitation; and, by various allusions and accounts, published of late years in Great Britain—prominent among which I may refer to an

[1] De l'Opération Médicale du Recrutement.

able lecture by Dr. Maclean, reported in the *British Medical Journal* of Feb. 1867,[1] in which the subject is incidentally mentioned—and in most of which the term irritable heart has been adopted. But, to this day, nowhere, whether as the result of the ordinary duties of the soldier or of actual war, has the subject, so far as I can find, been made one of careful clinical investigation. It is very possible that from inherent circumstances our war furnished more material of the kind than is likely soon to be met with again; for so many men called, by the tap of the drum, from civil pursuits, and sent without previous training into the field, is not a state of things likely often to happen. Whether among the Southern armies the same affection was common, I am unable to say; though from some facts that have been mentioned to me, I think it was. And it would be strange indeed, if men of the same race, transformed into soldiers under much the same circumstances, and, though operating oftener on interior lines, enduring on the other hand generally more privations, should have escaped.

So much by way of introduction. Let us now look at the medical aspects of the question.

GENERAL CLINICAL HISTORY.—The general clinical history of many of the cases was this:—

A man who had been for some months or longer in active service, would be seized with diarrhœa, annoying, yet not severe enough to keep him out of the field; or, attacked with diarrhœa or fever, he rejoined, after a short stay in hospital, his command, and again underwent the exertions of a soldier's life. He soon noticed that he could not bear them as formerly; he got out of breath, could not keep up with his comrades, was annoyed with dizziness and palpitation, and with pain in the chest; his accoutrements oppressed him, and all this though he appeared well and healthy. Seeking advice from the surgeon of the regiment, it was decided that he was unfit for duty, and he was sent to a hospital, where his persistently quick acting heart confirmed his story, though he looked like a man in sound condition. Any digestive disturbances which might have existed gradually passed away, but the irritability of the heart remained, and only very slowly did the excited organ return to its natural condition. Or it failed to do so, notwithstanding the use of remedies which control the circulation; thus the case might go on for a long time, and the patient, after having been the round of hospitals, would be discharged, or, as unfit for active duty, placed in the Invalid Corps.

This may be stated to be a general summary of a considerable number of cases. But there were many others originating more suddenly, or without previous digestive disorder, presenting also marked disturbance or irregularity of the circulation, and having also the pain in the cardiac region well

[1] See Medical News, May, 1867, p. 65.

developed. I shall cite a few cases of irritable heart by way of illustrating these remarks:—

CASE 261. *Irritable heart, previous marked diarrhœa; amelioration only of symptoms.*—William C., private, 140th N.Y. Vol., twenty-one years of age, single, and a farmer before he volunteered. He enlisted August 27th, 1862, had diarrhœa for about three months, and subsequently, while on a march from Harper's Ferry to Fredericksburg, had his attention drawn to his heart by attacks of palpitation, pain in the cardiac region, and difficulty in breathing at night. He remained, however, on duty, though not of very hard kind, until December 24th, 1862, when in consequence of a severe cold which resulted in loss of voice, he was ordered from the front. Transferred from hospital to hospital, he was sent June, 1863, to the U. S. A. Hospital, at Turner's Lane, where I examined his case. This note expresses his condition at the time:—

"Height 5 feet 6 inches; weight 145 lbs.; measurement around chest, one inch below nipple, 35 inches; appearance that of fair health; gums rather spongy, says that they bled easily while in the field, and he thinks he had a slight attack of scurvy; appetite good; bowels regular; respiration 24 in the minute; pulse 122; impulse of heart extended, and very jerky; first sound decidedly deficient; second sound distinct; percussion dulness not increased; capillary circulation defective; hands bluish. On lying down, pulse becomes fuller, and is reduced to 98, and after a few minutes, to 90; on rising, after lying for some time, it only very gradually regains its previous velocity. He is still aphonic, probably from catarrhal laryngitis; was etherized before I saw him, but without effect on voice. He has had lately about two nocturnal discharges a week, which has been the case with him for years, excepting when in the field and suffering from diarrhœa, when he was free from them. Has occasionally spells of dizziness."

He took, for a time, the tincture of gelsemium, but with little effect; veratrum viride, five drops of the tincture three times daily, and subsequently six drops daily, had a much better action. It reduced the pulse to 84, without materially altering the jerky character of the cardiac beat, and it had to be discontinued on account of the nausea. It was then given occasionally, while oxide of zinc, followed by strychnia, was prescribed. This, too, produced little permanent effect. November 16, he returned from a furlough, apparently in excellent health, but with an impulse of 98, and still suffering from palpitations, especially at night. He was now placed on digitaline granules, and, as it was evident that he would not be fit for active duty for a very long time, he was detailed as orderly. Under Morson's digitaline, $\frac{1}{60}$th of a grain three times daily, the action of the heart improved considerably. Yet when he was recommended in May, 1864, for the veteran reserve corps, he still had some cardiac pain and occasional palpitations. These, however, scarcely occurred excepting on strong exertion. It may be added that on the previous 1st of September the man regained his voice after a violent coughing spell.

CASE 87. *Irritable heart, chiefly from hard service; recovery.*—Wm. Henry H., private 68th Pennsylvania Vol., admitted into the Turner's Lane Hospital in Philadelphia, November 2d, 1863, having just returned from a furlough. He enlisted in August, 1862, at the time in good health, though he had suffered occasionally from rheumatism. He did a great deal of hard duty with his regiment. Some time before the battle of

Fredericksburg, he had an attack of diarrhœa; after the battle, he was seized with lancinating pains in the cardiac region, so intense that he was obliged to throw himself down upon the ground, and with palpitation. These symptoms frequently returned while on the march, were attended with dimness of vision and giddiness, and obliged him often to fall out from his company and ride in the ambulance. Yet he remained with his regiment until July 4th, 1863, when he was wounded at the battle of Gettysburg. The wound healed in about one month; but the cardiac symptoms became worse, and violent palpitations ensued upon the slightest exertion, sometimes also whilst in bed, obliging him to rise. There was soreness in the cardiac region, and a constant dull pain. The impulse was extended, slightly jerky, 96, and of irregular rhythm, some beats following each other in rapid succession; the first sound was feeble, the second very distinct. The man did not look sick. Height 5 feet 7 inches; measured 31 inches around the chest one inch below the nipple; he did not smoke; chewed tobacco in moderation.

The patient did not improve under aconite; but under digitalis the impulse became quiet and 78, and on March 23d, having previously done duty as orderly, he was detailed on police duty, and his treatment stopped. The heart continuing to act regularly, he returned to his regiment May 3d, 1864.

These cases represent the cardiac malady most commonly encountered among soldiers. But side by side in the ward I soon noticed cases of hypertrophy of the heart, such as one meets with in civil practice; cases with all the forcible action of an increased organ, with its physical signs, its usual symptoms, often with a comparatively slow and laboured action; cases, therefore, wholly unlike the persistent excitement of the rapidly-beating irritable organ. Holding at the time the common belief that functional and organic affections are widely separate, I failed at first to seize the fact that the apparently dissimilar states were in reality one, or rather, that one grew out of the other. But as patients multiplied I began to trace the connection; and observation showed me what I trust to demonstrate in this paper, the links connecting the disorders.

Having given some illustrations of the purely functional malady, let me by way of contrast here bring forward a case of undoubted hypertrophy, one among many (as Cases 22, 38, 75, 191, 264, 266, 269, 286), and by no means the one in which the enlargement reached its highest degree.

CASE 297.—*Hypertrophy following irritable heart.*—Wm. H. S., age 17, enlisted in the infantry in October, 1863. He was a lad who used tobacco freely, and at times liquor; but he was quite well at the time of enlisting, and served with his regiment for one year, doing much marching, and being much exposed. For a couple of months before he left it, he was frequently attacked at night with smothering or suffocating sensations, and with palpitations; and even prior to this had found it difficult to do his duty, and had signs of cardiac distress. Examined October 27th, 1864, I observed the impulse to be extended and forcible, beating 80 times in the minute; the first sound was heavy; the second only at the base very distinct. The cardiac percussion dulness was decidedly increased; he had only occasionally sharp pain over the heart. He improved somewhat

under treatment; but when he was discharged in May, 1865, the physical signs were unchanged; the large percussion dulness was not in the least modified, and the first sound was noted as heavy and dull at the apex; an occasional systolic blowing sound heard over the left ventricle had ceased to be perceptible.

But to return to the purely irritable heart.

SYMPTOMS.—Having indicated the general history of the malady, let us look in detail at the symptoms, taking for analysis merely the cases which were really of functional kind, or at least did not present decided organic change. And in the whole examination to follow, the remarks apply to this class of cases, unless the contrary is distinctly stated.

To investigate the palpitations first.

Palpitation.—Both the severity and frequency of the palpitations differed considerably in individual cases. In some (as in Case 208), the attacks lasted several hours, and were attended with increased pain in the cardiac region, and under the left shoulder. They were often accompanied by a great deal of distress, and were really painful. They occurred at all times of the day and night, varying in frequency from one to five or six attacks, or more, in the twenty-four hours. Yet there were cases that did not have them for days at a time. The seizures were, of course, most readily excited by exertion, and might be then so violent, that the patient would fall to the ground insensible (Case 119). This happened to some on the march (Cases 8 and 45), or field of battle; or they fell in the ranks, and were taken prisoner (Book V., p. 45, and *ib.* p. 56). But attacks also occurred when the patient was quietly in bed, disturbing his rest, or waking him up (Case 116); and some reported that they were worse at night (Cases 64 and 65), and early morning (Cases 111 and 230). They were very variously, sometimes whimsically, described. The rapid action was often commented on; but a "slow, hard" beat of the heart was also spoken of (Case 202); and one soldier (Book V., p. 45) likened the cardiac derangement to the "fluttering of a chicken, when taken by the legs."

The fits of palpitation were not only associated with cardiac uneasiness and pain, but in some with headache, dimness of vision, and giddiness. As a rule, the patient could not lie on his left side, for fear of exciting them; but there were those who could lie as well (Case 263), or better on the left side than on the right (Case 253), or who could not lie on either the right side or back (Case 118).

Cardiac Pain.—Pain was an almost constant symptom. I cannot recall a single well-marked instance of the complaint in which it was wholly absent; and often it was the first sign of disorder noticed by the patient. It was generally described as occurring in paroxysms, and as sharp and lancinating; a few likened it to a burning sensation (Case 111), or spoke of it as tearing (Case 122), or as burning at times, and at others

cutting; or as a "dull sullen" pain, becoming at times acute (Case 69). In some cases no other pain happened than what occurred in these sharp attacks (as in Case 91), or a mere feeling of uneasiness in the region of the heart existed; but in the large majority there was a substratum, as it were, of discomfort, or of dull heavy pain. In exceptional cases the pain was altogether of this character. Unwonted exercise or exertion would generally produce an attack of sharp pain, and a fit of palpitation was very apt to do the same; but the acute pain also happened without any unusual disturbance of cardiac action, and was, in truth, in rare instances, noticed to be decreased by exercise, or (Cases 92 and 257) to be most severe when the patient was free from palpitation. Deep breathing was stated to make the pain severe, when it was otherwise but slight (Case 15); cough produced (Case 86) a kindred result.

The chief seat of the pain was the lower part of the præcordia, particularly near the apex. But it was not always limited to the region of the heart. It was spoken of as shooting to the left axilla, as passing down the left arm (Case 111), which then felt numb; as being present under the left scapula (Case 239); and as radiating from over the heart in all directions (Case 240). The pain was associated with sensitiveness in the cardiac region, and this hyperæsthesia was apt to be increased after attacks of palpitation. In cases which ended in recovery, both pain and tenderness gradually left.

The pain was not due to intercostal neuralgia. Thus, in Case 92, I find this description of the pain, "constant heavy feeling over outer and lower part of cardiac region, and slight soreness. Occasionally pain becomes sharp, and when sharp flies to back, and at times to the head. There is also a slight pain, not, however, persistent, an inch or two above inferior angle of scapula. But there are no painful spots over the spine, or in the course of the intercostal nerves, and which might be attributed to intercostal neuralgia."

The latter affection undoubtedly, in some instances, existed, but it was as a complication. Just as pain in the back was occasionally encountered, due to excretion of abnormal ingredients with the urine, to muscular hyperæsthesia, to sprains, and to the many causes which give rise to pain in the back in soldiers.

Pulse.—The pulse was mostly noted to be very rapid, varying from 100 to 140. In character it was small, and easily compressible; it might or might not exhibit the abrupt or jerking character, which, as we shall presently see, is one of the chief peculiarities of the cardiac impulse, and this might have a certain amount of force which the pulse would lack. In some cases it was under 90, and was then apt to be fuller; these were, for the most part, the cases passing into cardiac hypertrophy. The pulse exhibits under any circumstances great variations; and especially in a case following an injury to the spine from a falling tree (Case 232) it changed

about between 76 to 120, little influenced by any remedy employed. Slight irregularities in the succession of its beats, and, indeed, in the general rhythm, are very common. The pulse is always greatly and rapidly influenced by position. Thus, in one case (Case 12), in which, in the standing posture, it was from 105 to 108, it became shortly after lying down rather less than 80, and fuller, and then gradually rose to 98; in another case (Case 68), it is noted at 124 standing, at 94 lying; in yet another case in which it often reached 140 to 156, but in which, just prior to the observation, it was 128, it was reduced to 82 in the recumbent position; in yet another (Case 193), it was counted as 120 standing, and as 84 when lying down. On the patient remaining for hours in the recumbent position, the pulse would in some cases slowly reach its minimum; in others it rose again by some beats after this posture had been for some time assumed; in all, the immediate effect of the exchange of position was most striking.

In the preceding remarks the pulse has been treated of when no palpitations existed at the time. These heightened it. But even when palpitation was not present, the beat at the wrist reached occasionally an extraordinary rapidity. Thus, in one case (Case 303), it was irregular, and seemed like a wave, slightly jerking and with intermissions; counted as accurately as possible, it was not under 192. Gradually, by rest and digitalis, it was reduced to 110.

I had several times the opportunity of studying the effect of acute maladies on the pulse, and I found that, instead of being rendered more rapid, it became slower. True, the recumbent position had something to do with the result, but the phenomena cannot be wholly thus explained. In one case (Case 227), the patient for some days before he was attacked with typhoid fever, and when in his usual condition, presented a pulse of 124, which became 96 and fuller on lying down. After he had been for nearly a week in bed with typhoid fever, and had already commenced to take stimulus, it was still only 96; it then for three days was 104, 108, and 100, and very feeble; for the subsequent two days 96; for the next five days it gave a record of 84, 84, 84, 80, 86, and was very compressible, notwithstanding twelve to fourteen ounces of whiskey were daily taken, with considerable quantities of milk and beef-tea. For five days after this record it was still 80, and the heart sounds were sharp and valvular, as they had been before the fever. Convalescence was now beginning, but when the fever had entirely gone, and the patient had left his bed, the heart's action again became about 120, and once, during an attack of palpitation, rose to 200. In a case of typhus in a soldier with irritable heart, there was much delirium, and the pulse was noted as resistant, but as only 96 (Book V., p. 35); in Case 138 the heart beat 90 habitually, 80 while under the influence of digitaline, but became reduced to 48 during an attack of catarrhal influenza.

While treating of the pulse, I may allude to some other peculiarities of the circulation. The eye was sometimes noticed to be injected, and the lips of bluish colour. The hands were often bluish, and other portions of the skin mottled, and rendered pale on pressure; in truth, the whole capillary circulation was often palpably defective.

Respiration.—Shortness of breath, or rather oppression on exertion, was constantly complained of, and was a prominent symptom during attacks of palpitation. When the heart was acting in its usual way, a certain amount of embarrassment in breathing was also commonly spoken of, and was at times so severe that the patient was obliged to sit up in bed. Yet, notwithstanding all the signs of dyspnœa, it was astonishing that the respiration was so little hurried. And, as a general rule, it may be stated that this curious disorder presents the anomalous condition that increased action of the heart does not give rise to increased frequency of breathing; we find in it a peculiar pulse respiration ratio, and one just the reverse from that of pneumonia. To cite as proof a few cases of irritable heart: In Case 143, the pulse was 124, the respirations 25; in Case 300, I noted a pulse of 146, and respirations only 26 in the minute; and in Case 303, a pulse of 192, respirations 26. In Case 9 I found an exception, for the pulse was 84, the respirations 32.

Nervous Disorders.—These manifested themselves chiefly by headache, giddiness, disturbed sleep; and were symptoms which, though common, were not so constant as those already described. The headache was not apt to be persistent, but to occur in spells, and was generally of a dull, heavy character. It was more particularly noticed after severe attacks of palpitation; and might (as in Case 149) be associated with giddiness, and with increased heat and redness of face.

Dizziness was often complained of. It was increased by stooping (Case 44); by exercise (Case 57); and sometimes preceded the attacks of palpitation (Case 62). In one instance (Case 265), the vertigo was so severe that the man fell from his horse.

Jerking during sleep and disturbed rest were annoying symptoms. The unpleasant character of the dreams was frequently referred to; one soldier spoke often of dreaming that he was falling off high buildings (Case 124).

The symptoms just referred to are all indicative of disturbed circulation in the cerebro-spinal centres. But there was also evidence of disorder of the sympathetic nervous system, as shown in the itching of the skin and excessive perspiration from which many suffered. Inordinate sweating of the hand was several times complained of (as in Case 159).

Digestive Disorders.—These were very frequent. All kinds of indigestions, great abdominal distension, and diarrhœa were symptoms constantly encountered. But they were symptoms having reference rather to the causation of the cardiac trouble than due to this; and we shall examine their bearing further on.

Urine.—There was nothing in this secretion of special importance; at least not so far as investigated in the usual manner. In some instances considerable quantities of oxalate of lime were found; and in others, the urine presented those variations which are common in the digestive disorders with which the affection of the heart was combined.

PHYSICAL SIGNS.—In describing the physical signs I shall first bring those together which are the most usual.

The *impulse* is almost always extended, yet not correspondingly forcible; rather, it is quick, and abrupt or jerky. When the hand is applied to the præcordial region, it may note the quick impulse happening in a regular manner, or it takes cognizance of the irregularity of rhythm of the irritable organ. Further, it may at times perceive the two sounds of the heart; feel them as it were (as in Case in Book V. p. 33). On listening to the heart, the first sound is found to be lacking in volume, feeble or short and valvular, and just like the second sound (Cases 112 and 136); and the valvular character of the first sound may be best appreciated a little to the left of the impulse (Case 285). As recovery ensues, the first sound gradually regains its usual character, and it may do this, although when it originally came under observation it was almost extinct (Case 170). The second sound of the heart is generally increased, and always very distinct; I met with but one instance (Case 147) in which it was the reverse. The sounds of the heart are freely transmitted beyond the cardiac region; the second sound may be heard very clearly defined over the carotid (as in Case 141).

Sometimes the sounds of the heart are split. Thus in Case 219 the impulse was very irregular; there were double beats and intermissions, and one of the cardiac sounds—the irregularity made it difficult to determine which, though I think it was the first—was curiously broken, and sounded like the sudden motion of an only slightly elastic or cartilaginous substance. Not even with a double stethoscope could a murmur be detected. In other instances (as in Case 121) the intermission every few beats now, or only every eighth or twelfth beat at another period, gave to the sounds following the perverted action at different times a different character.

But even where the rhythm is regular we meet with other modifications of the sounds. Thus, the first sound may be dull and not deficient, and this although, as in all the preceding cases, there is no increase in the cardiac diameters to denote enlargement of the organ. Nay (Cases 271, 273), the sound may be dull and heavy, the impulse rather forcible, and yet percussion of the heart not indicate hypertrophy. Still these cases end, I believe, oftener in this way than those in which the more typical signs of the irritable heart are present.

Murmurs obscuring or replacing the cardiac sounds are not as a rule present; yet they are met with, and particularly is that form of murmur,

systolic, chiefly above the apex, and not connected with venous hum or other signs of anæmia, which I have described (see this Journal, July, 1869) as significant of functional valvular disorder. It has all the peculiarities there dwelt upon; in truth, it was a study of the cases now under analysis that first familiarized me with it; and until I found out its meaning I was often much puzzled to know whether I was dealing with a case of organic valvular trouble or not. The inconstancy of the blowing sound is of much value; in cases of perverted rhythm it may only be heard with the first beat succeeding the intermission (Case 303).

COURSE OF THE DISORDER.—Having discussed the symptoms and physical signs, it will be useful to inquire into the course of the malady. This mostly either gradually subsides, or it passes by degrees into cardiac enlargement.

When the disorder yields, the heart becomes less and less irritable, exercise no longer affects it so much, the cardiac pain and soreness disappear, and finally the patient is again able to bear fatigue and undergo exertion; or in other instances, he is well as long as he is not too active, but his heart is always liable to be more disturbed by undue exertion or by excitement than the heart of a healthy person is. I shall give some illustrative cases, selecting partly those that returned to their regiments, but chiefly those that I had opportunities of observing after they left the service.

CASE 34. *Irritable heart; recovery; return to regiment.*—Here the cardiac symptoms were very marked in June, 1862, the pulse 96 and feeble, the impulse jerky. He gradually recovered, the impulse lost its jerking character, the first sound of the heart gained in volume, the rhythm of the heart became quite regular, its action much slower, and by August 5th, he could take exercise with impunity. After a furlough he was fully able to return to his regiment.

CASE 47. *Irritable heart; return to regiment.*—This case, when seen in July, had an impulse jerky and extended, a pulse of 100 and irregular; a feeble, indistinct, first sound. By September his pulse when quiet was 66, and became only 96 to 100 after running on the double quick; a slight hesitation was still observable with some of the cardiac beats. He was subsequently absent on furlough, and did guard duty; and before being sent to his regiment in December, bore active exercise well; the heart beat about 68 times in the minute, and was regular.

CASE 299. *Irritable heart; permanent recovery; patient seen several times since close of war.*—John H., private, 138th Penn. Vol., 20 years of age, a printer before enlisting. Joined the army in August, 1862; had while in service occasional palpitations and shortness of breath, but these symptoms did not interfere with the discharge of his duty until after an attack of fever contracted in August, 1864. When I saw him in November, 1864, he presented a typical instance of irritable heart. The impulse was most markedly jerky, slightly extended, not forcible, and the first sound short and valvular; the second sound increased; the pulse abrupt, 120, moderately full. There was a great deal of cardiac pain, dull,

aching, and referred to near the apex; the nights were restless; some digestive disorder was noted. He was of very temperate habits, and did not use tobacco.

Under a treatment of tincture of digitalis, carried to fifteen drops three times daily, and occasionally united with gentian, the cardiac symptoms gradually yielded; and in four months the impulse had become quiet, was 88 when taking no medicine, had almost entirely lost its jerk; the first sound had gained in strength, the second was still distinct, and he had no palpitations excepting on strong exertion. He went home on furlough, and came back still further improved. It was noted that the first sound was of good volume, not in the least valvular. Being now near the end of the war, he was discharged from the service in May, 1865.

I saw him again January 23, 1866. He looked in excellent health, and told me that he felt so. He had been employed as conductor on a street car, and suffered no inconvenience, excepting if he ran or lifted heavy weights. The first sound of the heart was of good volume, not valvular; the second sound was not too distinct; the impulse was perfectly regular, though not very strong; the pulse of good volume, 80.

I had another opportunity of examining this man in March, 1870. He was in every respect very well; the pulse was 84, the second sound perhaps a little more distinct than normal, but the heart's action was excited by his having walked rapidly. He never had palpitation or shortness of breath, or cardiac pain, excepting at times on heavy lifting; was able to walk fast, and had not long since walked thirty miles on the stretch.

CASE 48. *Irritable heart; permanent recovery; does duty in the Veteran Reserve; patient seen very recently.*—Joseph W., enlisted in the 12th New Jersey Infantry in 1862, when 17 years of age. Was not strong, and picket duty and marching soon told heavily on him; he commenced to suffer with dizziness, with cardiac pain, shortness of breath, and palpitation. The long march from Falmouth, Virginia, to Gettysburg, broke him down completely; on the second day of the march he threw away his knapsack and extra clothing, and could even then scarcely keep up with the regiment. He was soon afterwards sent to a hospital in Philadelphia, where I saw him July 13, 1863. The pulse was 104, very feeble; the impulse jerky, and felt in two intercostal spaces; the second sound was somewhat exaggerated, the first only moderately distinct. The man's weight was 128 pounds; height 5 feet 5 inches; measurement of chest 27½ inches. He improved under a treatment of digitalis, veratrum viride, and iron, given singly, and sometimes suspended to treat troublesome attacks of cough, to which he was subject. But as he was a weakly-looking lad he was detained in the hospital, doing light duty, and March 13th he was recommended for the Veteran Reserve, 1st battalion, as his physique did not warrant his being sent into the field. But the heart difficulty had yielded, the impulse had lost its jerk, and the first sound was of good volume. His first duty before he joined the Veteran Reserve battalion was guard duty in Philadelphia, and I had an opportunity of examining him June 29, 1864. The impulse was 70, regular; he had no longer palpitation, and only occasionally cardiac pain.

Early in November of 1870, just as this paper was going into the hands of the printer, a tall, broad-chested, fine-looking young man came into my office, in whom at first I did not recognize the puny soldier of 1864. He told me that he served his term out, of about one year, in the Veteran Reserve, first at Washington, and then for seven months in the interior

of Ohio. Here he was on detached service, on the provost guard, was mounted, lived very constantly in the open air, and did sometimes hard riding. On one occasion he ran his horse for nine miles at full speed in the pursuit of a prisoner, but neither this nor any other service brought back the cardiac disorder. In fact he was never off duty, had neither pain nor shortness of breath, and only twice an attack of palpitation, happening after a "bilious spell." He weighed 142 pounds when his term expired; and it is evident that he could then have re-enlisted for active service in the field. Since he left the Veteran Reserve has been most of the time a house-painter. Never has anything now that reminds him of his former trouble. Can go up and down stairs rapidly, or run, without being soon out of breath; weighs 127 pounds; still takes cold occasionally. The pulse is about 78; the impulse of the heart of good strength, not in the least jerky, is still felt in two intercostal spaces; the first sound is of good volume, if at all different from the normal average it is rather heavier; the second sound is not of exaggerated distinctness; the percussion dulness is normal.

CASE 62. *Irritable heart; recovery; return to active duty; seen subsequent to return.*—Samuel J. L., aged 23, enlisted in 1862, never was able to do much marching, and had marked cardiac symptoms subsequent to an attack of remittent fever; felt much beating of the heart after the double-quick. Had an occasional cough and night-sweats. I saw him August 22, 1863, six months after he had done active duty. The note speaks of him as "the picture of health, gums firm, digestion good, not the least appearance of scurvy, uses no tobacco. Cardiac impulse extended, somewhat forcible, 104, irregular, some beats slow; first sound confused, second somewhat increased." He improved slowly, taking veratrum viride, then digitalis, and wearing a belladonna plaster for the relief of the cardiac pain. In February, 1864, his impulse was 86, and had become regular. In March it was 76, only the most trifling irregularity could be discerned, and he had no longer palpitations. He went to Washington to be examined for a captaincy of coloured troops. The board did not object to him on physical grounds, and he passed successfully. He joined his regiment, and marched with it for several months, when I again saw him, while he was on leave of absence. He had remained well, all irregularity had ceased. The first sound was distinct, and of good volume; the impulse was 68, perhaps slightly stronger than a normal impulse.

CASE 69. *Irritable heart; recovery; does heavy marching after return to duty; seen subsequently.*—Samuel P. C., 1st Delaware Infantry, aged 23, enlisted September, 1861; cardiac difficulty manifested itself in the autumn of 1862 while marching, and obliged him to go off duty. But he returned after a month, finding, however, much difficulty in keeping up with his regiment. An injury received at the first battle of Fredericksburg was the cause of his being sent from the front. He had much pain over the heart; the impulse was 118, jerky and extended, the first sound very deficient, the second very distinct, the cardiac diameters were not increased. This record was made September 8th, 1863. January 8th, 1864, the impulse was 82, but he was still taking digitaline granules. February 8th, 1864, the impulse was 72 and quiet, and the digitaline had been gradually withdrawn. He was shortly afterwards recommended as fit for duty, but as he obtained a commission in a coloured regiment did not remain long with his old comrades. I saw him January 19th, 1865; he

had been acting as captain, drilling and marching his men, but as yet had not been sent to the scene of hostilities. He was in good health, and free from cardiac pain or palpitation. The impulse was 70, of fair force, not jerky; the first sound still, perhaps, slightly enfeebled.

In July, 1866, I met him again. He told me that he had been in the service until January, 1866, had seen much active service before Richmond and in Texas as captain and lieut.-colonel. Had done much heavy marching, but without return of his cardiac symptoms. The heart was of normal size, the first sound distinct, and of good volume. He had been walking a good deal before I examined him, and the impulse was 96, and slightly stronger than normal, but he assured me that this was quite exceptional.

All the cases that I have thus far described terminated in perfect recovery; and in the last two, there was the test of actual service in the field to prove how complete the recovery had been. I shall now give examples of amelioration, but of irritable condition of the heart remaining when the patients were last examined.

CASE 184. *Irritable heart; great improvement, yet traces of the disorder when last examined.*—Nelson F., 93d New York Vol. In this case the derangement of the heart was first noticed after a fever, though before he had done a good deal of hard service. The heart's action was not particularly rapid, 92 was its highest, and usually it was 80; but it was extremely irregular, and there was considerable soreness in the cardiac region. He improved much under treatment, especially under digitalis, but being, in consequence of an injury to his arm, considered unfit for duty, he was discharged. I saw him six months afterwards; he had been travelling and acting as nurse; he still had palpitation occasionally on exertion, and a slight double beat of the impulse could at times be perceived; yet for the most part, the action of the heart was regular; the pulse was 78.

CASE 7. *Irritable heart; recovery not complete when last seen.*—P. S., 82d New York Vol., had diarrhœa at Harrison's Landing, in August, 1862, but did not notice any cardiac trouble until in September, after the battle of Antietam. He remained, however, on duty for several months, until he received a slight wound in the shoulder. Examined in April, 1863, I noted him as having pain over the heart, and occasionally palpitation, especially after exertion; pulse of moderate volume; impulse somewhat abrupt, not extended; first sound ill developed, lacking in weight; second sound very distinct. He was placed on digitaline, and had improved considerably when, in obedience to a general order, he was sent to his regiment. January 31, 1868, he came to see me at my office. He told me that he had not been able to do duty with his regiment, and that he had been mostly on provost duty, until his time expired in June, 1864. He has been since only rarely troubled with palpitation, but much exertion still makes him feel weak, and somewhat short of breath. Yet he can go up stairs without exciting inordinate beating of the heart, has no cardiac pain, and considers himself as well. The first sound of the heart was still short, dull, and confused; the pulse was 72, compressible, the beats not quite regular.

Here then was a case in which an all but complete recovery ensued, a slight cardiac derangement being, however, traceable for years. In the

preceding case it was also discernible, though not for so long a time, and it is very likely that this ended in recovery. This happened in a man (H. K., Book V. p. 72) who was mustered out of the service at the end of the war, still having palpitations occasionally, though he had greatly improved, and who has lately told me that for two years his heart continued to give him some trouble, yet now he is well and a hardworking carpenter.

To examine now an entirely different result of the irritable heart, a group of cases in which *the disordered function leads to organic disease.*

CASE 41. *Irritable heart; hypertrophy results.*—James A. E., private 136th New York Vol., enlisted Sept. 1862; when first seen, in June, 1863, had clearly an irritable heart, which had resulted from fever and hard duty. The attacks of palpitation were very frequent, and he could not walk across the ward without bringing one on and increasing the cardiac pain. The pulse ranged between 98 and 118, was feeble, occasionally intermitted, and the beats were of unequal strength; immediately on lying down the pulse became fuller, and fell to 72. The impulse of the heart was abrupt and extended; the first sound deficient, the second very distinct; percussion dulness was of normal extent.

This case proved very unyielding to treatment, and though the action of the heart became slower, the palpitations did not cease. Very likely the cardiac irritability was kept up by repeated attacks of cough, to which he seemed very liable. He was in April, 1864, put in the Veteran Reserve, 2d Battalion, having done duty first in charge of a ward; he served while in the Veteran Reserve on guard duty, always remaining subject to palpitation on strong exertion, and the impulse being of moderate force.

I met him in August, 1865, and though he said he was well excepting when he fatigued himself, it was evident that the heart had become hypertrophied; the impulse towards the apex was more forcible than normal, and the first sound heavier; the second sound had lost its increased distinctness; the transverse percussion dulness measured $4\frac{1}{2}$ inches.

CASE 73. *Irritable heart; hypertrophy as a consequence.*—Edward K., 114th Pennsylvania Volunteers, enlisted August, 1862. Suffered in the army a great deal from constipation and dyspeptic symptoms, had also rheumatism, and chewed much tobacco; he was, therefore, in a condition in which a cardiac malady would be readily produced, and in about six months after joining his regiment attacks of palpitation were so frequent and violent that he had to leave it. I examined him in September, 1863; he was a weakly-looking man, ill nourished; the gums were spongy and bled easily, but there was no eruption on the skin. The impulse was 92, somewhat extended, rather abrupt, but could hardly be called jerky; the first sound was very decidedly deficient. The man took mainly ergot, gentian, and nitrate of silver in succession, and much attention was paid to his digestive system; but no treatment was very rigorously carried out, as he was mostly employed as orderly in another part of the hospital. March 14, 1864, he was recommended for the Veteran Reserve; he appeared much better; the impulse was 84 and less extended, the first sound of decidedly better volume, but he still had cardiac pain and palpitation on exertion.

In July, 1870, I met this man, and found that the case, which had not been apparently a very bad one of irritable heart, had become a decided one of dilated hypertrophy. The transverse diameter of cardiac dulness

was 4½ inches; the impulse extended and forcible, the apex beat lowered; the first sound was indistinct, prolonged, murmurish. He complained much of shortness of breath, and is still troubled with palpitations; sometimes they occur when he is quiet, and he is perfectly unable to undergo any great exertion; the cardiac pain, however, left some years ago. He grew worse after leaving the army, and while attempting to work actively at his trade of a baker, he became much weaker, and had frequent smothering sensations. Latterly he has been on a farm, taking care of horses, and suffers much less.

CASE 90. *Irritable heart, followed by hypertrophy.*—Benjamin V. T., 114th Pennsylvania Volunteers, age 25. The cardiac difficulty was noticed while in active service, but was aggravated after he was taken prisoner and suffered from an attack of rheumatism. Nov. 8, 1863, the impulse was 112, jerky, slightly extended; the first sound was sharp, short, valvular; the second accentuated. The pulse, easily compressible, had something of the jerk of the impulse; the cardiac diameters were normal. Aconite was of no use; digitaline did better, and in April he was placed on guard duty, still taking the remedy occasionally, but it was certainly interfered with by his intemperate habits. In June, 1864, he joined the Veteran Reserve Battalion No. 1; he still had palpitations at times, but no cardiac pain excepting on exertion; the impulse had lost very much of the jerk; the first sound was longer and less valvular, though still deficient. The heart's action ranged between 88 and 92.

He remained in the Veteran Reserve Corps up to March 28, 1865, doing light duty. His heart continued to trouble him; he was short of breath on exertion, and had attacks of palpitation. I examined him in October, 1866. The impulse was felt in several intercostal spaces, was forcible, and 96. Both sounds could be perceived by applying the hand to the cardiac region; the first sound was heavy, and occasionally at the apex murmurish; the second was not accentuated. There was evidently hypertrophy, though not in the most marked degree; the transverse diameter measured 4 inches; the perpendicular, 3⅝. The feet were not swollen; and though he had shortness of breath and strong beating of the heart on active exercise, there was no constant cardiac pain.

CASE 40. *Irritable heart; beginning hypertrophy before placed in Veteran Reserve; subsequent increase of enlargement and development of valvular disease* (?).—Henry R., age 29, had been, when I saw him in June, 1862, six weeks off duty on account of pain in the cardiac region and increased action of the heart. The pulse was 84, impulse forcible with only a moderate jerk; the first sound was not faint, though short. The cardiac diameters were increased—transverse, 3½ in.; longitudinal, 4 in.; oblique, 4¼ in.; height of man, 5 feet 4 in.; measurement around chest, one inch below nipple, 31 inches. The case was looked upon as one of beginning hypertrophy; and after the action of the heart had been favourably influenced by aconite, the man was placed in the Veteran Reserve, 1st Battalion. He did duty chiefly in cities, in the provost guard, but I was informed that he was very intemperate in his habits. Two years ago I encountered the man; he had most marked cardiac hypertrophy, and a systolic murmur at the apex which I believe to have been organic, though unfortunately I had but one opportunity of examining him, and cannot give this case as a positive one of valvular disease following the greatly increased enlargement, though I believe that such was the case.

All these cases, excepting in part the last, were cases in which the enlargement of the heart occurred after they passed from under my hands and mostly after their return to civil life. But I find among my records instances in which the change took place in spite of treatment, and while the patient was actually under observation, or had been so most of the time. Such a one was Case 225, which is described as a very decided example of irritable heart, July 18, 1864, with an abrupt impulse, beating 124 times in the minute; a short first sound. October 13 the first sound is noted as somewhat stronger; the transverse diameter of the heart is $2\frac{3}{4}$ inches; the impulse is unreduced in frequency. The man was withdrawn from my control for upwards of a month; the impulse is then stated to be more forcible, and the heart is kept much disturbed by recurring malarial outbreaks and by intemperance. May 19, 1865, when the last note is taken, the case is one of undoubted dilated hypertrophy; the first sound is rather heavy, the second less distinct; the impulse is 108, extended, somewhat forcible; the transverse diameter is 4 inches. A similar record is made in Case 236; while in others, (as in Case 145) men who are not steadily under treatment, or who obtain furloughs, or are sent away in obedience to general orders, to meet exigencies, are found at first with forcible hearts and just beginning enlargement, which on different opportunities for examination is observed to have become much more developed.

In the light of these cases we can interpret others. We can understand how it happened that Case 185, a man who had been in sixteen fights, and for two years had not been off duty a day, though gradually more and more troubled with palpitation, and much distressed while carrying his knapsack on long marches—how such a man, who was often obliged to halt and sit down until the spell had passed off, but who loved the excitement of campaigning so much that he would not leave his regiment—we can well understand how it was that, giving his heart no chance for recovery, he should have presented all the signs of most marked hypertrophy. We can further understand cases sent to the hospital when this state was beginning, and in which the irritable heart was just passing into, or had just passed into, the enlarged heart. Such a case, for instance, of which the following represents one of many:—

CASE 160. *Irritable heart passing into hypertrophy.*—George L., private in the 4th Delaware Vol., 34 years of age, and a morocco dresser before joining the army. He enlisted August 12th, 1862. After being some time in the field, he found himself unable to do much marching or to stand wearing duty, and, after a heavy cold in the spring of 1863, while doing picket duty near Yorktown, was obliged to leave the regiment. He was transferred from hospital to hospital, taking a variety of remedies, and regarded as a case of cardiac disorder with general debility. Before entering the army he drank freely and chewed tobacco in excess; but in both respects his habits while a soldier improved very much. When admitted into the Turner's Lane General Hospital, in Philadelphia, his lips

were of good colour, his gums healthy, but the general appearance was not that of a very strong man, nor had he, indeed, ever been very robust. He had attacks of palpitation, very rarely, however, excepting under excitement or on exertion; and only very seldom pain over the heart. Dyspnœa was readily produced, and in consequence of oppression, he was sometimes obliged to sit up in bed at night. The digestion was good, the bowels were rather relaxed; he had had indeed diarrhœa, though not when the trouble was first noticed. There never had been palpitations prior to enlisting, though at times, on strong exertion, he had been short of breath. During his stay in the hospital, from May until October, 1864 (excepting in August, when absent on furlough), he had occasional attacks of giddiness. Save when under the influence of decided doses of aconite, the impulse was rather forcible and extended, beating from 96 to 100 in the minute; the first sound was not enfeebled, but somewhat more distinct than usual; the cardiac diameters, measured after being some time under treatment, were: perpendicular 4; oblique $4\frac{1}{2}$; transverse $3\frac{1}{2}$ inches. The man's height was 5 feet 7 inches; the measurement around the chest $32\frac{1}{4}$ inches. He was discharged in October.

As to the exact state of the heart in these "mixed cases" or cases of beginning hypertrophy, I can give other evidence, than that from mere clinical observation. I had the opportunity of inspecting the organ.

CASE 85. *Irritable heart, beginning hypertrophy; death from strangulated hernia; autopsy.*—John A. R., 143 Pennsylvania Vol., age 19, a farmer, enlisted September 9th, 1862. About four years before had an attack of typhoid fever, but experienced no cardiac difficulty until February, 1863, when he began to be troubled with pain in the region of the heart, with palpitation and dimness of vision. He had done a great deal of hard picket duty; and, during the spring and summer, marched considerably, and also suffered from diarrhœa. The trouble with his heart now greatly increased, and as in addition he received an injury, he was obliged to leave the regiment.

Examined November 5th, he is thus noted: Weight 135; height 5 feet 6 inches; measurement around the chest, one inch below nipple, $34\frac{1}{2}$ inches; appearance that of perfect health; dull pain near apex, and occasionally sharp shooting pains; often attacks of palpitation, especially at night; complains much of headache; pulse of moderate strength, 96. The impulse is somewhat extended, but not decidedly abrupt, and of some force; the second sound is very distinct; the first, as compared with the second, appears indistinct, but it is not valvular; the rhythm of the heart is almost regular. Not much change took place under conium; but aconite had an influence in lowering the action of the heart, though he still suffered from palpitation.

January 12th, 1864, an inguinal hernia he had became strangulated; after vain attempts at reduction, an operation was skilfully performed by Dr. Hall; the pulse under ether was 96, after the operation 98. The man, notwithstanding the great attention he received from Dr. Hall and Dr. Thomas B. Reed, U. S. V. in charge of the hospital, did not long survive.

Autopsy about twenty-four hours after death.—All the organs were healthy, and there were no signs of disease excepting those connected with the hernia. The pericardium was healthy; the heart, before being opened, appeared to be of about normal size; at its upper portion was a moderate

amount of fat; the valves were all healthy; a small clot was entwined in the mitral valve; the auricles were of normal size, and so were the cavities of the ventricles; the muscular structure of these was firm, and the cut surface glistening. While, as already stated, the cavity of the left ventricle did not appear increased, there was a great disproportion between its walls and those of the right side; these measured less than $\frac{1}{4}$th of an inch at some parts, a fraction over $\frac{1}{4}$th at the thickest portion; whereas, the walls of the left ventricle were nearly $\frac{1}{3}$th of an inch at the thickest part, and varied from a little over $\frac{1}{3}$ to $\frac{2}{3}$ths of an inch at others. Microscopically examined, the fibres were healthy, some fibres seemed indistinct, but there was neither fatty nor granular degeneration. The nervous filaments of the heart, as far as they were traced out, appeared healthy, but no minute dissection of the ganglia of the heart was made.

Now taking the usually given average of over one-sixth of an inch for the right ventricle, and about half an inch for the left, we see that the former was only slightly increased, whereas the latter showed decided signs of hypertrophy having begun. And while discussing these cases in which the functional passes into the organic malady, we find these to be the manifestations: the impulse is extended, becomes perhaps more so, but loses its abrupt character, has more volume, and begins to be forcible; the first sound lengthens, its valvular character disappears, and so does the great distinctness of the second sound; the action of the heart becomes gradually slower, the pulse fuller; the cardiac pain lessens, and the respiration bears a more natural proportion to the still excited action, but the breathing is more persistently oppressed. The percussion dulness at first still admits of doubt in explanation, but gradually the boundaries become markedly extended. What becomes of these kinds of hearts? Is the hypertrophy ever arrested? I believe it is. But I have already discussed this point, which belongs to the consideration of organic, rather than of functional affections, in a paper contributed to the *Medical Memoirs of the U. S. Sanitary Commission* (New York, 1867), in which the whole subject of cardiac enlargements, including those growing out of disturbed action, is considered. I will here merely add a few words as to the relative frequency of the affections. In 200 cases carefully examined, there were 28 undoubted instances of hypertrophy; 136 of irritable heart; and 36 doubtful or mixed cases, in which the functional disorder was passing into the organic malady, and in which every variety of gradation in the direction of signs and symptoms just indicated was met with (as in Cases 16, 42, 56, 88, 123, 169, 202, 234, 284).

DIAGNOSIS.—The diagnosis of the affection has become evident from the preceding remarks, and I shall confine myself here to a few general statements, chiefly about matters that have not been explained or touched upon. And first, of the cases last discussed, and which were viewed as cases in a state of transition to hypertrophy. It is necessary to mention that in

exceptional instances they are simulated by irritable hearts, in which the first sound is heavy, and the impulse rather forcible; but the percussion dulness is not increased, and whether we view them as cases arrested at the very beginning of organic change or not, it is certain that (as in Case 77) the heart returns apparently to a perfectly natural condition. In some instances of the kind it may be necessary to watch the effect of treatment—which, generally, rather rapidly influences the disordered organ—before a positive conclusion can be reached.

As regards the examples of hypertrophy we must bear in mind that fixing the boundaries of the heart only once is not sufficient for diagnosis, for increased dulness on percussion may be temporarily caused by engorgement of the cavities with blood. Again, both with reference to cases of hypertrophy and of irritable heart, we must remember, as above explained, that blowing sounds from excitement and functional valvular disorder are very common; and must guard against confounding the cases, on account of these murmurs, with valvular disease of the heart. I may add that I have known many a man discharged the service for organic valvular disease, who presented merely the combination of phenomena here referred to. The similarity may be greatly heightened by the coexistence of dropsy, a sign I very seldom noticed in the cases of hypertrophy, still more seldom in those of irritable heart; but which does happen in either, and when coexisting with a cardiac murmur may perplex the most skilful diagnostician. In Case 187 this combination was present, but all the signs disappeared and the man recovered. The dropsy of the irritable heart is limited to the lower extremity, and is apt to be very transitory.

Dilatation of the heart I encountered only very rarely. Where it existed it might have been confounded with irritable heart; but the extent of the percussion dulness, and the marked tendency to dropsical effusions were very significant features. Anæmic hearts are distinguished from irritable hearts by the history, by the murmurs at the base, the venous hum, the aspect of the patient, and the absence of cardiac pain. Occasionally the two affections are combined, but I have left out these instances in the analyses on which this paper is based.

It would seem scarcely necessary to speak of the differential diagnosis between irritable heart and phthisis. Yet they may be mistaken. Slight irritative coughs or attacks of bronchitis are not uncommon, perhaps more common than in persons whose circulation is not disordered, and with the disposition to shortness of breath and the rapid pulse may well mislead. Moreover, soldiers with irritable heart as well as with hypertrophy, may spit blood after fatigue or violent exertion, and thus the resemblance is strengthened. But the aspect of the patient, the pain in the præcordial region, the attacks of palpitation, and the absence of the physical signs of tubercle furnish the distinctive traits.

Lastly, it is impossible to discuss any malady to which soldiers are

liable without discussing its being feigned. And a malingerer, as is well known, may keep up a rapid action of the heart by a tight bandage around the upper part of the abdomen and lower part of the chest. But, excepting if this be done, the imitation is a very clumsy one. The impostor knows nothing of the character of the cardiac pain. Further, making him lie down after undressing causes the heart to return to its natural beat; and then on his resuming the erect position, or walking around quietly, it will not, as an irritable heart does, regain its former frequency or irregular rhythm. And the peculiar physical signs of the irritable organ, and its persistently rapid action when examined under any circumstances save when the patient is in the recumbent posture, are traits which cannot be copied.

CAUSES.—In discussing the causes we are led to examine some of the most interesting questions connected with this inquiry. But in no part of it is it more difficult to arrive at fixed conclusions, for many causes seem at times to have combined, and it is scarcely possible, even by the most rigorous analysis, to fix specially upon one. In the subjoined table great care has been exercised to arrive at the probable causing element. The cases which have served as its basis have been only so far selected that doubtful or ill-marked ones have been excluded, and that those patients who were chosen, were for the most part in good general health.

Analysis of 200 *Cases.*

Fevers	34	17 per cent.
Diarrhœa	61	30.5 "
Hard field service, particularly excessive marching	69	38.5 "
Wounds, injuries, rheumatism, scurvy, ordinary duties of soldier life, and doubtful causes . . .	36	18 "
	200	100

This table requires some words of explanation. As regards the *fevers*, 29, or 14.5 per cent., were typhoid fever, or the so-called typho-malarial disorder, or pure remittent fever; only, however, very few of the latter: the other 5, or 2.5 per cent., were measles. And among the fever cases some noticed palpitation and shortness of breath on exertion during convalescence; but in the majority the cardiac symptoms did not become marked, certainly did not attract attention, until after return to duty.

Sixty-one cases of *diarrhœa*, or 30.5 per cent., represent certainly more than a mere coincidence. Yet even this number might be increased to about 35 per cent. if we take into account other gastro-intestinal affections which have been included in another category, such as obstinate constipation with occasional jaundice (Case 3), or greatly swollen tympanitic abdomen (Cases 33, 147). In a few of the cases only in which diarrhœa was thought to be the exciting cause, did the looseness of the bowels

persist, or was it at least still readily excited by indiscretions in diet; in most it had ceased for a considerable time before the patient came under observation.

Hard field service was the chief assignable cause in 38.5 per cent. of the cases. Some of these had also had gastric or intestinal disorder, though not of persistent or active type. But constant and heavy duty on the picket line, or during active movements in the face of an enemy would develop the cardiac symptoms; or slight before, these increased and became marked after forced marches (as in Case 110 after a march of twenty-six miles in one day); or during arduous and exciting fighting and marching (Case 119, who noticed it first after fighting and marching for three days, sleeping on his arms, and being greatly depressed in spirits during Gen. McClellan's retreat in front of Richmond). In some instances (as in Case 129) the history was given of heavy marching having caused the cardiac disorder, but of diarrhœa having greatly aggravated it.

The cases that are here under examination belonged to infantry, cavalry, and artillery, though in greatest numbers to the former. The fact that they did not belong to this exclusively is conclusive that the form of *accoutrements* was not the exciting cause of the affection. Undoubtedly the waist belt, but particularly the knapsack, may have had something to do with aggravating the trouble; but I could find no proof that they had produced it. We must indeed be very careful in accepting what a soldier says of his equipments. His knapsack especially is an annoyance to him—a kind of *bête noire*, which he likes to hold responsible for his mishaps. And many is the man who believes or affects to believe that but for the impediment it has been to his action, he might, on some occasion, have secured the promotion which some more favoured comrade did. Modern armies are in this respect not like ancient armies. It is no longer, as with the Romans,[1] a boast of the overwhelming loads they carry; nor is the soldier of the nineteenth century so overweighted that carrying, like the legionary, in addition to his arms and provisions many tools, he can be described, as his prototype was by Josephus, as differing little from a mule of burden.

We come now to the last category in which 36 out the 200 cases stand, and here we meet with wounds, injuries, rheumatism, scurvy, and various other causes. I could find but extremely few instances in which a *wound* had even seemed to be the starting point, though in some the disorder was aggravated by it, particularly if on return to duty they saw at once much active service. *Strains and blows*, and similar injuries, had a much more direct influence. Case 240 was knocked insensible by a falling tree; Case 241 was prostrated by a sand-bag striking him over the heart; Case 249 was, in a hand-to-hand encounter, hit on the left breast with the butt

[1] Cicero, Tuscul., ii.; Liv. Epit. LVII.

of a musket; Case 287 strained his back while carrying railroad iron at the battle of Weldon Railroad.

Rheumatism was the undoubted cause in some instances (as in Cases 17, 20, 104, 109). The rheumatic affection unquestionably also attacked the muscles of the heart, and under continued exertion, or on going back to duty soon after the seizure, the cardiac malady was manifest. In one case (Case 154), there had been rheumatic attacks before enlisting, and a slight cardiac derangement, which became a very annoying one on the man's going into active service.

With reference to *scurvy*, I found a condition of things very different from what might be anticipated. To say that it existed, or that there had been symptoms making it highly probable that it had existed, in five per cent., is to accord the fullest possible allowance. Judged by its usual symptoms, of spongy gums, of spots on the skin, of pseudo-rheumatic pains, it was a very rare causing element; and the long continuance of many of the cases, even after they had been home on furlough, had lived on the most varied diet, and while looking robust and well, does not make it seem as if a scorbutic state of system had had much to do with producing them.

Sunstroke I find assigned as the probable cause in two cases (Case 288, and in Book V. p. 66). In one case (118), the cardiac malady appeared to have followed pneumonia, though a closer analysis made it much more likely that it ensued upon hard riding, before full convalescence. Then there was a small proportion of cases—not amounting to more than three per cent.—in which the causes were so complex as to defy analysis, or in which the trouble seemed to have originated without their ever having been in the field, or done arduous duty.

While examining into the causes of the affection, the influence of some must be mentioned which have a bearing as predisposing causes, or as aggravating the malady. One of these is *tobacco*. That it was not the real source of the malady, is proved by some of the worst cases occurring in those who did not use it in any shape (Cases 102, 108, 118); yet it seemed to me that those who had chewed or smoked in excess (Cases 127, 159), were more liable to the severer forms of the disorder; or that these habits aggravated it (Case 106). Frequent *seminal emissions*, too, predisposed to it, or kept it up (Case 59); but from a careful inquiry into this point, I am satisfied that neither these, nor venereal excess, nor masturbation, nor syphilis, produced the disorder. Some of the cases were rendered prone to the affection by having suffered occasionally with shortness of breath or palpitation *before enlisting* (Cases 153, 193), and some of these had had rheumatism. In all these cases it was apt to be noticed that, from the onset, the double-quick was badly borne.

Age acted also as a predisposing cause, as seen by this table.

Age in Two Hundred Cases.

Age.	Cases.	Per cent.
From 16—18 years	24	12
" 18—20 "	26	13
" 20—25 "	86	43
" 25—30 "	34	17
" 30—35 "	17	8.5
" 35—45 "	13	6.5

Fully two-thirds of the cases happened from the 16th to the 25th year. Here the majority occurred from the 20th to the 25th; yet I think that if the question were considered with reference to the number of persons under 20 years of age in the ranks, these would be found to have furnished the largest percentage.

Occupation appears without influence on the production of the malady. Painter, butcher, blacksmith, carpenter, the city-bred man who had left his desk in the counting-house, the farmer fresh from tilling the fields, were all fully represented in the long list of sufferers.

When now we take a comprehensive view of the general causes of the malady, we find quick and long marches, heavy work, producing the affection, or even slight exertion in those whose constitution has been impaired by insufficient or indigestible food, or whose strength has suffered, or condition of the heart been disturbed by diarrhœa or fever. We find it most readily developed in those previously weak and unaccustomed to fatigue, or subject to readily-quickened circulation. We find it kept up by irksome equipments, and other causes, but not generated by them.

But in looking further, and in endeavouring to explain the *nature of the malady*, there is room for much doubt and difference of opinion. It seems to me the most likely that the heart has become irritable, from its over-action and frequent excitement, and that disordered innervation keeps it so. But our knowledge of the nerves of the heart, and their special functions, is as yet too incomplete, to say which nerves lie at the root of the disturbance. It appears most probable that the special nerve centres near the base of the heart which preside over the normal rhythmical movements of the organ are stimulated, and in considering the close connection of these ganglia with the sympathetic, we have the explanation of how their function may have been stimulated, or kept so, by irritation reflected to them from the abdominal ganglia or elsewhere. Further, the pain may be due to hyperæsthesia of the cardiac nerve centres; and its radiation is readily accounted for by the connection between the cardiac nerves through the cervical ganglia of the sympathetic, from which the nerves of the cardiac plexus are derived, with the roots of the cervical nerves and branches of the brachial plexus. As evidence how the sympathetic system participates, might also be cited the obvious disturbance of the capillary circulation and the perspirations.

RESULTS.—To look now at the results, I do not mean the purely medical results which we have already examined while investigating the course of the disease, but rather the results in so far as they concern the soldier.

Result in 200 *Cases.*

	Cases.	Per cent.
Returned to regiment	76	38.
Detailed for other service than with regiment	13	6.5
Time expired	11	5.5
Uncertain	63	31.5
Veteran Reserves, I.	18	9.
" " II.	5	2.5
Discharged	14	7.
	200	100

The 76 who returned to their regiment, did so for the most part only after they had done guard duty for some time, and had been thoroughly tested. Of a number I heard subsequently, and they were able to serve well.

The 13 who were detailed for other service than with regiments, as clerks, orderlies, etc., were chiefly such who for some ailment besides the cardiac malady were judged unfit for the field. Of the 11 whose time of enlistment expired, about one-half would have been sent back to their regiments; and in the same proportion would probably have been the disposition of the 63 marked as of uncertain result. This category embraced men who by special order were transferred to their respective States, or to other hospitals, deserters, men whose remaining term of enlistment was so short that it was evident they could not in that time have been fit for duty, and some in which the definite destination was not ascertained, or not mentioned in the notes. In the Veteran Reserves those were placed who could do some kind of duty in garrison, etc., but not in the field; while those who were discharged were cases of which it was certain never would be of much use to the service; some, however, of these were discharged prior to the formation of the Invalid Corps, or Veteran Reserve.

TREATMENT.—Under this head I shall first examine into the effect of rest.

Rest.—After what has been said of the great change in the frequency of the pulse produced by the recumbent posture, it cannot be a matter of astonishment that rest in this position was found to have a very decided influence. Indeed, were it not for the almost impossibility of enforcing this mode of treatment in soldiers who, except for the rapid beating of the heart, do not consider themselves as sick, we might by merely confining them to bed achieve valuable therapeutic results. In a number of the cases I made them lie down several hours daily, and generally with strikingly good consequences. In the following instances circumstances made the recumbent position a necessity, and its influence can be distinctly traced

CASE 19.—*Irritable heart ameliorating under rest.*—A cavalry soldier with irritable heart, which had occurred subsequent to a fever. The first sound was somewhat decreased; the impulse varied between 98 and 104. During an attack of dysentery, mainly treated by opiates, it was 98, and then 88; but on remaining in bed for two weeks after the attack was over it fell to 78; on leaving the bed it never again rose to its former frequency; and the man in about three weeks more was able to return to his regiment.

CASE 4.—*Irritable heart improving by rest made compulsory by a fracture.*—An infantry soldier in whom the cardiac disorder followed chronic diarrhœa. The attacks of palpitation were frequent; the pulse ranged between 98 and 120; while under cardiac sedatives the lowest it reached was 86. The man became intoxicated, fell, and broke his leg. After he had been for some days in bed, and not taking medicine, and his leg in a splint, the palpitations happened very rarely, and the pulse was 72. Unfortunately the case now passed from observation by being removed from the hospital.

Next let us examine into the action of remedies which are supposed to act specially on the heart, and which are mostly regarded as cardiac sedatives.

Digitalis, digitaline.—I gave both these articles an extensive trial, taking care to procure pure specimens. I used them on at least one hundred patients, either alone or as the chief agent in the treatment. In the researches about to be recorded I shall refer only to the cases in which no other agent was employed. I may further premise that in an individual case either a good tincture or the digitaline was employed; but never both at the same time. Many of the observations were made with a tincture specially prepared from selected leaves, or with Morson's digitaline. On the whole, I may say that I saw no decided difference whether digitalis or its active principle was employed; and that both had more influence on the cardiac disorder than any drug which was resorted to.

In slight cases digitalis lowered the pulse and made it steadier in about one week, and gradually led to permanent improvement, as was shown even after it was withdrawn. In severe cases its action was slower, but finally as decided. In Case 11 eight drops of the tincture given twice daily reduced the impulse from 114 to 85, and made it less jerky. And in changing the character of the impulse and rendering the first sound more distinct, the influence of the medicine was often very perceptible. Thus in Case 186 a $\frac{1}{60}$th of a grain of digitaline given three times daily, produced in a week a marked change, and subsequently given four times daily, having been stopped for a time, reduced the impulse from 100 to 80, and nearly removed its jerking character. In Case 184 there was a very intermitting pulse which became nearly regular under the almost persistent use of tincture of digitalis (U. S. Pharm.), given at one time in doses of fifteen drops three times daily. In Case 291, a cavalry soldier, in whom the cardiac disorder originated after much hard service, the impulse was noted Octo-

ber 19th, as 120, jerky, the first sound short, somewhat valvular; there was considerable cardiac pain. Fifteen drops of tincture of digitalis were given three times daily; on the 26th the impulse was 108. The treatment was then broken in upon by a furlough, from which he returned with an impulse of 96, but it was resumed and carried on for upwards of three months, the dose being reduced after a time to ten drops three times daily, with the result that the impulse lost its jerk entirely, never exceeded eighty, that the first sound no longer presented a valvular character, and that the cardiac pain had almost disappeared. In a case (recorded Book V. p. 52) the impulse when first examined was irregular and 160; after using ten drops of the tincture three times daily for nearly a month it was noticed as stronger, without irregularity, and 90.

Digitalis often compared very favourably with other remedies. In Cases 14 and 15 it proved far better than either gelsemium or ergot; in Case 22, than veratrum viride; in 18, better than strychnia; in Case 119 it succeeded when conium and nitrate of silver had failed; the same in Case 64, where neither aconite nor veratrum gave any results; and in Case 41, though it did not fully answer, it was much more beneficial than either.

These were all pure cases of irritable heart. In cases with forcible impulse simulating enlargement, or those in which hypertrophy had already commenced, cases which I have above described as uncertain or mixed cases, the action was sometimes useful, but often doubtful or negative. Thus in Case 271 it reduced the frequency, but not the force of the action of the heart; in Case 234 both digitalis and veratrum viride were of use; in Case 202 where aconite and veratrum did good, digitalis was useless, also in case 77; and in Case 261 a two months' treatment with it produced no effect, while aconitia in about one month, in $\frac{1}{30}$th of a grain doses three times daily, reduced and quieted the forcible impulse, brought it down from 116 to 80, and caused the disappearance of a blowing sound which the previously excited heart presented. In Case 90, both aconite and digitaline failed; while the latter succeeded in Case 127. In cases of hypertrophy the action of the drug was generally still less advantageous than in the mixed cases (as in Case 264).

But it must not be inferred that even in the purely irritable heart it was always successful. In Case 219, in which the impulse was irregular and 120, fifteen drops of the tincture were given for two weeks without any effect. A subsequent treatment by $\frac{1}{60}$th of a grain of digitaline four times daily, carried out for three weeks, modified the impulse by only ten beats, and had no effect on the irregularity; nor had thirty drops of the tincture given three times daily an effect on either, a slight effect perhaps on the frequency, but not on the irregularity. In Case 228, though a treatment with digitalis and digitaline was persevered in for months, the impulse remained about 120, the only modification being that it was less abrupt.

The doses in which digitalis or digitaline were habitually given, have become evident from the cases referred to; 10 drops, three times daily, of the tincture; and from $\frac{1}{60}$th to $\frac{1}{30}$th of a grain of digitaline, represent the average amounts; and occasionally these doses were continued, with slight intermissions, for weeks, or even months. The diuretic influence of the medicine was sometimes very palpable; in some cases its persistent administration seemed to dilate the pupil. Of its supposed cumulative effect I saw no proof. Indeed, is not this effect rather caused by the drug not being carefully watched, by its having begun to reduce the circulation, and then each dose telling with greatly increased force on the lowered heart? As regards the mode of action of digitalis, there may be evidence adduced from the series of observations here recorded, that its influence is largely on the nervous ganglia of the heart, and that through them it regulates the movements of the organ. Its so-called tonic properties would then be, to a great extent, the secondary consequence of its controlling power over the cardiac nerves.

Aconite.—The effects of this drug were very clearly defined. On purely irritable hearts it had very little; nay, it was repeatedly noticed that the impulse became more frequent under its use (Cases 87, 116), and even more abrupt (Case 89). But its action on cases in which the impulse was forcible was valuable (Case 169), and hence its power in some of the mixed cases was strikingly shown (as in Cases 137, 140, and 307. In the latter case aconite reduced an extended, moderately forcible impulse from 108 to 80). And it was a very curious fact that aconite might in these mixed cases act markedly on the force of the contraction of the heart, while it little influenced its frequency (Cases 117, 191), and this sometimes (Case 144) where digitalis had precisely the reverse effect. Nay (Case 287), it was noted as continuing to reduce the force steadily and most decidedly, while given in about two minim doses, three times daily, for twenty-eight days, while the frequency of the pulse was 96 when it was begun, and 96 when it was stopped. A similar observation was made in Case 235, where aconitia was employed three times daily, at first in $\frac{1}{60}$th of a grain doses, then in $\frac{1}{30}$th. And it may be remarked that these were the doses of aconitia usually employed, while the average dose of aconite was one to two minims of a carefully prepared tincture of the root of officinal strength (three times as strong as the British), administered two or three times daily. In a few cases, however, this dose was doubled.

If, then, aconite showed its effect in reducing the force of the heart and lessening the tension of the pulse, while, as we have found, digitalis quieted the excited organ and reduced its frequency, it was a natural suggestion that their combination might effect both purposes, and act in a manner which neither alone could. And so, indeed, it was repeatedly found by actual observation (as in Case 241). Further, experience soon taught which remedy was likely to succeed best; and whether from mere

functional excitement, or from beginning organic change, the cases with more or less forcible, extended, not abrupt, impulse, with the first sound not valvular or deficient, were placed on aconite, the progress of the case generally proving that the judgment was correct.

But it was after all in cases of decided increase of the organ, in cases of hypertrophy, that aconite most showed its influence. No medicine could then be compared with it, and when steadily, and in moderate doses, used for months, the effects were remarkable, even, I think, a decrease of the enlarged organ happening.[1]

Veratrum Viride.—This active drug has a better effect than aconite on irritable hearts, though not so good a one in cases of hypertrophy. It stands, as it were, between aconite and digitalis. In Cases 57, 60, 61, and 68, it answered as well as this could have answered; in Cases 22 and 208 well, but not so well as digitalis did. These were all instances of irritable heart. In those of the "mixed" kind it proved better (as in Case 38), than digitalis. As a rule, however, in cases of irritable heart, even where it succeeded well in calming the cardiac phenomena, its action was not apt to be so permanent as that of digitalis, though it was rendered permanent in some instances where it was combined with tonics, as in Case 39 with zinc, or in Cases 34, 46, 47, 48, 55, with iron. To show how little permanent—when given alone—its influence may be, I shall allude to two cases. In one (Case 15), though the heart was greatly quieted, the disorder returned when the veratrum viride was stopped; in another (Case 26), it was used for four months, but when abandoned, the disturbance showed itself again.

Veratrum viride was mostly employed in drop doses of the fluid extract, or in from five to ten drops of the tincture, three times daily. One specimen of the tincture used was of Dr. Norwood's own make. By giving it with tincture of ginger, its nauseating effects were readily obviated, as they may be also by brandy in small quantities, without thus interfering, to any extent, with the action of the veratrum on the circulation.

Gelsemium.—With this article, which is supposed to have properties as a cardiac sedative, I was much disappointed. It had for the most part no effect at all (Cases 25, 27, 33, 34), or the effect was uncertain and transitory. In some instances, when given in decided doses, it produced unpleasant symptoms. Thus, in Case 195, in which, when the observation was made the impulse was 96 and rather abrupt, the fluid extract of gelsemium, given in five-drop doses three times daily, for eight days, rendered the action of the heart somewhat feebler, without reducing its frequency; at the same time the legs and hands became shaky, there was roaring in the ears, dimness of sight, with dilated pupil, a sense of weight above the

[1] See cases reported in the paper in the Medical Memoirs of the U. S. Sanitary Commission above referred to.

eye, and dizziness. The bowels were not disordered, nor was the urinary secretion. In Case 267, with an impulse somewhat forcible, and a first sound not deficient, one drop of the fluid extract given three times daily, for a week, led to no result; three drops three times a day for the same period rendered the impulse slightly less forcible, but did not reduce its frequency; the patient stated, however, that he slept better. Administered then in five-drop doses for three days, it caused enlarged pupils, and dimness of vision, without materially altering the force of the cardiac impulse, and diminishing the frequency only by a few beats. And in cases of decided hypertrophy it was also of little use; far inferior to both veratrum viride and aconite. In Case 269, one of hypertrophy, it produced pain above the eye, dimness of vision, and enlarged pupil, without any marked reduction of force, or other change in the impulse of the heart.

Belladonna: atropia.—Of belladonna, a very different statement can be made than of gelsemium. It proved a most efficient agent. It was particularly valuable in instances of irregular action. Thus, in Case 63, $\frac{1}{60}$th of a grain of atropia given twice daily, for a week, made a permanent change in an irregularly acting heart, without having much effect in influencing the frequency. A similar result was observed in Case 99, where the same dose given three times a day, for sixteen days, gave back to the heart perfect regularity of movement, while lowering its frequency by ten beats, quieting the pain, and greatly lessening the palpitations. It did not do this, however, until the constitutional effects were fully produced, and kept up for about a week. Nor did, on suspending the remedy, the heart stay materially reduced in frequency, though the general symptoms remained favourably modified, and the rhythm of the heart regular. In Case 100, the jerking character of the impulse permanently changed under atropia, and a real improvement took place in all the cardiac symptoms, without the frequency of the heart being much altered. In Case 105, a heart intermitting every fourth to sixth beat showed in five days of atropia treatment no longer any intermissions, and after the medicine was stopped did not again present any decided irregularity.

In some instances, however, atropia influenced both the frequency of action and the irregularity, as in Case 106, or in Case 104, in which it produced remarkable effects in five days, but was not kept up long enough to lead to permanently beneficial results. In examples of irritable heart in which no irregularity existed, but merely rapid action, it rarely showed itself of decided use, nor in those with forcible impulse (as in Case 115). Indeed, I thought that in some instances it increased this and the pulse. Atropia was repeatedly used hypodermically for the relief of cardiac pain, and almost always produced marked, yet only temporary, alleviation (Cases 65, 69, 87).

On the whole, then, the action of belladonna and atropia was most clearly shown on the element of irregularity, and in relieving certain symp-

toms. It was often a valuable adjunct to treatment, or most serviceable when followed or combined with digitalis or iron (Case 179).

Atropia was usually given in the place of belladonna, and after the active principle had been administered for four or five days, the well-known constitutional effects were plainly discernible. But though the granules were then given but twice, or once daily, they were for some time continued, the dryness of the throat being the most troublesome symptom complained of, and occasionally interfering with sleep, which otherwise was apt to be less disturbed while taking the remedy.

It may not be uninteresting to record here some observations made when atropia was used hypodermically. They do not differ essentially from what happens in healthy persons.

CASE 173. *Irritable heart; hypodermic injection of atropia.*—John A. P., 18th Pennsylvania Cavalry, contracted a cardiac disorder while in service, which was greatly aggravated after an attack of fever. This case, when I saw it, had lasted rather more than a year, counted from the first manifestations, which happened while doing duty. He had a steady dull pain in the cardiac region, palpitations on exertion, and often at night. The impulse of the heart was jerky, slightly extended, 96; the first sound very feeble, the second extremely distinct.

June 14, 1864. After he had rested for a few days the impulse and pulse were counted as 98, at 5.41 P. M.; the other signs were the same as recorded; a scant $\frac{1}{30}$th of a grain of atropia was injected hypodermically. 5.50, impulse somewhat increased in force, 94; pupils uninfluenced; dryness of throat. 6 P. M. pulse 120; both it and impulse have decreased again in force markedly; pulse is indeed rather feeble. 6.18, pulse 122; impulse, compared with its usual condition, less jerky; respiration 25; complains much of dryness of throat; pupils dilating. 6.50 P. M. pulse 111, weak; pupils dilated; is dizzy and weak; has frontal headache; respiration 24. 7.55, pulse 99; respiration 27; symptoms about the same. 8.43 P. M. pulse 78; respiration 24.

15*th*, 8.30 A. M. Pulse 85, full and strong; respiration 25; pupils not dilated. 11.15 A. M. impulse 86 (lying down 76), is not so jerky, and pulse fuller than before injection.

17*th*. Impulse (standing) 96, certainly less jerky; and less pain and fewer palpitations.

CASE 195. *Irritable heart; hypodermic injection of atropia.*—James H., 17th Pennsylvania Cavalry, the cardiac disorder followed chronic diarrhœa and hard riding. Has pain over heart; palpitation; impulse 114, jerky, first sound short and valvular; pulse jerky, easily compressible. Injected (aided by Dr. Keen) $\frac{1}{30}$th grain of atropia at 5.30 P. M., June 14, while patient in lying position. Pulse 118; other signs the same as just mentioned. 5.37, pulse 108. 5.44, pulse 138, not increased in force; pupils large; throat dry; feels dizzy. 5.53, pulse 146, seems deficient in force; impulse, though it is confused, has lost its jerk; respiration 29; pupils very markedly dilated. 6.15, pulse 148, still feeble; impulse has certainly lost its jerk; respiration 30; much dryness of throat; no headache, but dizziness; limbs seem to him numb, and the general sensibility appears slightly impaired. 6.55 P. M., pulse 105; respiration 21; pulse

still weak, and general weakness exists. 7.57, pulse 97; respiration 24. 8.47, pulse 14; respiration 27; pupils still much dilated.

June 15, 8.40 A. M. Pulse 98, weak; respiration 22. Was wakeful and dreamed much during the night; throat no longer dry, but peculiar taste; pupils somewhat dilated. 11.28 A. M., impulse 94 (still lying down), is 102 standing. Yet has some pain over cardiac region.

17*th*. The impulse is 108, has regained its jerking character, though it is somewhat less jerky than before the injection.

This patient was subsequently placed on atropia internally. It had a good effect on the pain and palpitation, but none on the frequency of the cardiac action. The jerk of the impulse, however, disappeared. It was observed in this case, as it had been in others, that the dilatation of the pupil was greater just after it had been fairly produced by the atropia, than days afterwards, notwithstanding the medicine was kept up. Digitaline, given after the atropia treatment, exhibited a very decided and permanent influence on the rapidly-acting heart, reducing it to 80.

Opium.—This was rather incidentally tested while prescribing for diarrhœa or some other affection in which it was indicated, than used persistently for the irritable heart; for in the long continuance of the treatment required there would have been great risk of making the patient an opium eater. As far as it was tested, however, it struck me as having some quieting action on the excited heart, but as not likely by itself to be of much service. Hypodermic injections of morphia were often resorted to for the relief of the cardiac pain, and almost invariably, at least for the time being, accomplished the desired object. Case 115 alone is recorded as not favourably influenced.

Hyoscyamus.—In the cases in which this was employed it did not seem valueless, yet did not lead to any decided results. The action of hyoscyamia (obtained through the kindness of Prof. Procter) was studied in two cases, of which this is an abstract.

CASE 58. *Irritable heart; action of hyoscyamia*.—A cavalry soldier, off duty for four months in consequence of an irritable heart, brought on by heavy duty on the Rappahannock. The impulse was particularly jerky, and the first sound very deficient, almost extinct; the pulse 118. Veratrum viride reduced it materially, but neither it nor digitaline produced any decided improvement.

Nov. 25. He had not taken any medicine for three days; the pulse was 108; the other signs as recorded. He commenced with $\frac{1}{30}$th of a grain of hyoscyamia three times daily.

December 14. Impulse the same; does not feel drowsy; sleeps about the same; bowels more regular, are indeed inclined to be loose.

17*th*. Impulse about the same; a little more frequent at times, 116; respiration 24; as much cardiac pain; bowels constipated; he has not, however, taken the pills steadily, and not taking any now; is ordered to take $\frac{1}{30}$th of a grain four times daily.

22*d*. Impulse 88; respiration 20; bowels slightly loose, two passages daily; skin normal; appetite good; is thirsty, and has no feeling of dryness

in mouth; pupils large, but are naturally so; no headache; cardiac pain still exists; feels drowsy during day and sleeps better at night.

30th. Bowels rather loose; some headache; muscular power unimpaired; pupils not dilated; sleeps soundly; has an excellent appetite; but cardiac pain is quite as severe, and palpitations as frequent. The action of the heart this morning is much excited, 118; the impulse, as previously, jerky. The medicine was now stopped. The patient was placed on atropia, which had much more effect on the palpitations, then back on digitaline followed by iron, when he commenced to do guard duty. Gradually the palpitations ceased, the impulse lost its jerk, and the first sound wholly regained its volume.

In the next case, which was not nearly so severe a one, the action of the remedy was more satisfactory.

CASE 80. *Irritable heart; action of hyoscyamia.*—Had an irritable heart with an impulse usually about 82, but irregular, some beats following each other very rapidly. He took $\frac{1}{60}$th of a grain of Morson's hyoscyamia three times daily for two days, and $\frac{1}{30}$th for eleven, and four times daily for five days. The same loosening effect on the bowels as in the preceding case was noted, though it was not quite so marked; on the other hand, the pupils, of which in Case 59, owing to their being naturally very large, it was doubtful whether they became dilated, were here markedly affected, and considerably dilated. The sleep was improved, and the man felt, even in the daytime, heavy, and had some headache, but his mind remained clear. There was no nausea, and the urinary secretion did not appear disordered. He thought at times that his throat felt somewhat dry.

The impulse of the heart became quieted, and its rhythm much more regular, and at one time it was reduced to 60. Nor were the cardiac symptoms ever again marked or severe. After the suspension of the remedy, the impulse is noted at 72, and regular.

Conium; Cannabis Indica; Valerian; Ergot; the Bromides.—Of these, Cannabis Indica had the most effect (as in Case 112), though with references to the bromides, I must state that, with our present experience on the subject, they were not tried in doses large enough (Case 262) nor with sufficient persistency. Valerian had been employed in a number of the cases before I saw them, never with any resulting good. It was also useless in my hands in Case 230. From ergot I obtained no result in Case 15, a doubtful result in other cases (as Case 14). Conium showed an effect in Case 88, and more than once seemed useful when combined with tonics (as with nitrate of silver in Case 95).

Tonics—I shall group the articles of this class together, because in the main the results arrived at with them were the same, and these results were, that they often proved most excellent adjuncts to treatment, were of some service by themselves in light cases, but that otherwise they were insufficient to control the disorder.

Of *strychnia*, this was observed in Cases 62 and 68, marked cases in which it was given without effect; and in Case 76, $\frac{1}{16}$th of a grain was

administered steadily for nearly a month without benefit. *Quinia* was carefully tested in Case 84, six grains being given daily, but without influencing the action of the excited heart; and the same was noted in other cases, in which larger doses were prescribed. From *arsenic* and *nitrate of silver* some good was observed, but the former gave repeatedly very doubtful results (as in Case 84); of the latter, it is recorded that it succeeded in one case (93). Several cases did well on *zinc* (Cases 21, 50), indeed zinc had certainly some controlling power over the affection. The same may be said of *iron* (Case 83), given usually in the form of the tincture of the chloride, and this remedy was often found very valuable when combined with digitalis, or given subsequently to it (Cases 179, 304,). In an anæmic man (Case 238) this remedy indeed proved ineffectual until so combined, and in some comparatively light cases (as in 151) following fevers iron alone was sufficient; while in a very pallid man (Case 130) an irregular heart became regular under its use.

The bitter tonics, particularly *gentian*, were often prescribed in instances with attending gastric disorder, either alone or as vehicles for digitalis or veratrum viride.

Acetate of Lead; Iodide of Potassium.—In cases complicated with diarrhœa, acetate of lead was several times ordered, and from the power which lead is said to have on the heart, the action on this was carefully watched. None was perceived (Cases 70, 79, the first was a case passing into hypertrophy). *Iodide of potassium* was prescribed in a case of syphilitic node (Case 87), one of the few instances in which syphilitic infection existed, but it had no influence on the irritable organ.

Having thus discussed how individual medicines act on the malady—and I may here state that I was very particular to analyze such cases only in which no other indications called for special treatment, or in which the effect of this could be separated clearly from that of the medicine employed for the cardiac disorder—a few words as to the treatment of some of the symptoms may be necessary. Hoffmann's anodyne was occasionally prescribed for the attacks of palpitation but strict rest in the recumbent position was more generally enforced. I have spoken of the utility of hypodermic injections of morphia or atropia for the relief of severe cardiac pain. A belladonna plaster persistently worn was often also productive of good; whereas, blisters and cups, and very many of the soldiers had had them applied before I saw them, proved useless. In Cases 20, 58, 248, which I particularly noted, blisters were valueless.

The treatment was usually a protracted one; it may be therefore asked, were not the rest and food rather than the medicine employed the chief agents of cure; and was it not the amelioration of the general health alone that caused any beneficial results? But I have already stated that most of the men when first seen were to all appearance healthy, and many

had been for some months away from the field. Moreover, I noticed repeatedly, that improvement in general health did not go hand-in-hand with improvement in the cardiac condition. Thus, in Case 263, a man with an irritative cough became robust under the use of cod-liver oil, but the heart showed no change. And Case 71 was watched for a month while under no treatment, the heart varying between 84 and 106 in that period, at times severe palpitation occurring. No real improvement was perceptible. But being then placed on digitaline for upwards of a week, and subsequently sent home on furlough, he got so well that he was able to return to his regiment. And it was often observed, that when in obedience to general orders men were furloughed before any amelioration had taken place in the cardiac action, they returned not improved, sometimes worse. But after improvement had fairly set in, furloughs were frequently granted with much advantage, and the heart, which had already to a great extent lost its irritability, righted itself (as in Cases 101, 131, 279 and 280).

Great care was taken with the men during convalescence. They were mostly placed on guard duty or other light duty, some still continuing treatment in a modified degree, others not; and were ordered up for examination at stated intervals. Before being sent back to their regiments, they were tested by running and other exercises with a view of seeing how the action of the heart was affected. I have above referred to some cases that I had the opportunity of examining after they had rejoined their regiment, and I heard of others; they were fully able to fulfil all required of them.

The treatment is never a short one; and the question arises, would it not be better for the government at once to discharge these heart cases? I think not. The very worst ones, those which after some months of treatment show no decided improvement, had better be discharged; so had those which happen in the very young, or in soldiers in whom some decided complicating ailment exists, which is likely to prevent the cardiac malady from entirely leaving; though even some of these can be kept as fit for guard duty, for service in hospitals, or in convalescence camps. But all other cases should be kept for treatment; and then either, if fit, sent back to their regiments, or passed into such organizations as the Veteran Reserve battalions during the late war, which were intended to do provost duty, to serve in fortified places, in fact, to do everything except active service in the field. And from these organizations again, should the war prove a protracted one, the cases that have thoroughly recovered may be sent to rejoin their former regiments. To act otherwise, to discharge the large number of cases of functional disorder of the heart, which must exist in every army during war, would, I believe, deplete it as much as an engagement, and have on many a soldier, seeing the ease with

which a discharge can be obtained, a demoralizing effect. As for the time required before the disposition of a case is made, this depends much on judgment in individual instances, and while, until I understood the malady, I retained a number of patients a long period in the hospital, later in the war a short time sufficed to make the proper disposition of them. I may add that most cases of hypertrophy were, as well as many of irritable heart, placed in the Reserve battalions after being subjected to treatment. The worst forms of the organic malady were discharged; but the kind of life they were to lead was always impressed upon them, so was the desirability of their being under competent medical advice.

In bringing this inquiry to an end, I may be permitted to point out what I believe to be its chief interest and value. To the medical officer it may be of service as investigating a form of cardiac disorder which every severe or protracted campaign is sure to develop. And from a military point of view, further, it enforces the lessons, how important it is not to send back soldiers just convalescent from fevers or other acute maladies, too soon to active work; it suggests that their equipments be such as will not unnecessarily constrict, and thus retard or prevent recovery; that recruits, especially very young ones, be as far as practicable exercised and trained in marches and accustomed to fatigue before they are called upon to undergo the wear and tear of actual warfare; and it exhibits some of the dangers incident to the rapid and incessant manœuvring of troops. True, on a movement executed on the double-quick may depend the issue of a battle, a forced march may determine the fate of a nation; and the time can never come when purely physical considerations can forbid, either one or the other, or dictate how often they may be ordered. But every commander should be made aware that in so using his men he is rendering some unfit for further duty, impairing others, and thus be led to count the cost of the frequent use of such active movements as carefully as he would the holding of a particular part of a line or the assault on another.

But the chief value of this inquiry is after all to the practitioner of medicine. It traces, I venture to hope that I may say establishes, the connection between functional derangement and organic change, and examines the intermediate steps. And it is a contribution, based on trials made on a very extensive scale, towards the accurate knowledge of the action of remedies on the heart; showing, among other points, how a remedy may specially influence one of the elements of disorder without affecting the others.

ART. II.—*A Critical Inquiry into the Management of the Perineum during Labour.* By WILLIAM GOODELL, M.D., Clinical Lecturer on the Diseases of Women and Children in the University of Pennsylvania. (Read before the Philadelphia County Medical Society, January 26th, and September 14th, 1870, and recommended for publication.)

THE ancients, so far as can be gleaned from their writings, gave no support whatever to the perineum during labour, but limited their treatment to forced dilatation, and to the application of emollients, among which warm olive oil took rank. Yet, among the "ten thousand disorders"[1] enumerated by them as peculiar to woman in her sexual capacity, it has been alleged that lacerations of the perineum find no place. Denman,[2] Velpeau,[3] and Leishman[4] contend that the first allusion to this accident is found in a fragment attributed to Eros, who flourished in the thirteenth century. To explain this alleged silence, Denman—an advocate for supporting the perineum—uses the following ingenious argument:—

"They [the ancients] did not therefore advise any method of preventing this accident, because they were ignorant of the cause, or they undervalued it. It may be further observed that the oldest writer on midwifery lived before the Christian religion was established, and in countries in which polygamy was allowed, when the death or infirmity of one wife was comparatively of little importance to him who had many, equally, or in some degree, dividing or partaking of his affection. But, on the establishment of the Christian religion, by which the selfish and bad dispositions of the human mind were intended to be restrained or corrected, and its better qualities exalted, one wife only being allowed to one man, and she being supposed to possess the entire affections of her husband, every disease or infirmity, which might render her person less agreeable to him, became of infinite consequence to their mutual happiness."

On the other hand, Leishman[5] adduces as an argument against supporting the perineum, that "it is a significant fact, however, that in no instance do we find them [the ancients] attaching an undue importance to laceration of the perineum, and, indeed, in most cases, they avoid all mention of it whatever."

Is it, however, a well-established fact that the ancients are silent upon this point? Four hundred years B. C., Euripides, in his tragedy of Medea, makes her exclaim to the Corinthian matrons, "I would rather run the risks of battle thrice, than bring-forth once"—ὡς τρὶς ἂν πας' ἀσπίδα στῆναι ξὺν ὁπλοιμ' ἂν μᾶλλον ἢ τεκεῖν ἅπαξ—evidently referring to the injuries she had received in giving birth to Jason's children. Hippocrates,

[1] Τῶν ἐν γυναικὶ μοχθων μυρίων παραιτίη μήτρη. Democritus ad Hippocratem de Naturâ Humanâ.

[2] Introduction to the Practice of Midwifery, chap. ix., sec. viii.

[3] Traité des Accouch., vol. ii. p. 637.

[4] Glasgow Medical Journal, January, 1860.

[5] Mechanism of Parturition, p. 87.

who flourished at the same period, asserts that "in countries exposed to north winds, parturition is difficult. * * * Frequently consumption attacks the women, determined by their labours, whose throes produce ruptures and lacerations"—*ὑπό γὰρ βίης ῥήγματα ἰσχουσι καὶ σπάσματα.*[1] This passage has, it is true, given rise to much fruitless discussion,[2] and Littré[3] deems these "ruptures and lacerations" to be pulmonary, and not of the maternal passages.

But I am the more inclined to the opinion that they refer to perineal lacerations, because women thus afflicted, by brooding and by secluding themselves, decline in health and become cachectic. Thus D. Sennertus speaks of a lady—beautiful, of course; the patients of the older physicians were always beautiful—who fell into a slow consumption and died from the effects of an ununited laceration of the recto-vaginal septum—"quæ tandem etiam, cùm ruptura hæc consolidari non posset, lentâ tabe obiit."[4]

Again, Hippocrates recommends an astringent treatment "when in labour much purging takes place, the womb, bladder, and gut being *together wounded*, and neither the feces nor the urine can be controlled, but escape involuntarily"—*Ην ἐν τόκῳ καθαρσις ᾖ πολλὴ, ἡ ὑστερη ξυνελκοῦται καὶ ἡ κύστις, καὶ ἔντερον, καὶ ὔντε το κόπριον κατέχουσι, καὶ τὸ ουρον, προιενται δε.*"[5] Such a grave accident is almost unknown at the present day, but Felix Plater, of Basle, describes an analogous case of vesico-vaginal fistula, conjoined with a complete laceration of the perineum. "*In partu violento, fœtu extincto, cuidam mulieri uteri et vesicæ cervix adeo læsa,*

[1] De Ære, Aquâ et Locis, cap. iv.

[2] Genuine Works of Hippoc., by Francis Adams, 1849, vol. i. p. 194.

[3] Œuvres d'Hippocrate, vol. v. p. 579.

[4] Practicæ Medicinæ; Wittebergæ, 1632; lib. iv. p. 6.

[5] De Morbis Mulierum, lib. i. apud Calvum, Ed. Aldes, 1526. I am bound to state that this passage is differently translated by many commentators, and that the weight of authority is against me. The whole difficulty hinges on the verb, which is not the same in all the MSS. Thus, Maurice de la Corde (Commentarius tertius, par. xiv. in lib. Prior. de Muliebribus, 1585), Föesius (Hippocratis Opera Omnia, Francofurti, 1621), Réné Chartier (Hippocrat. et Galeni Opera, Paris, 1639, vol. vii. p. 757), Vander Linden (Hippocrat. Opera, Ed. 1665, p. 457), Haller (Artis Medicæ Principes, Lausannæ, 1770, lib. iii. p. 191), and Littré (Œuvres d'Hippocrate, tom. viii. p. 80) adopt the verb *συνέλκεται*, from the barytone *ἕλκω—to contract together*. On the other hand, the Greek text of Calvus (Aldes, 1526)—the earliest of all the editions, and based on the Vatican MSS.—gives *ξυνελκοῦται*, from the contract verb *ἑλκόω—to be wounded or torn open*. This last reading is adopted by Gardeil (Traductions des Œuvres Médicales d'Hippocrate, Toulouse, 1801, vol. iv. p. 115), and by John Redman Coxe (The Writings of Hippocrates and Galen, Philadelphia, 1846, p. 296). Granting, however, that the contract verb *ξυνελκοῦνται* is a gloss, still it must be one of great antiquity, for Calvus consulted the oldest and most authentic manuscripts within his reach.

ut excrementa confusa stercoris et urinæ invicem redderentur."[1] Aristotle—350 B. C.—clearly alludes to these lacerations in his work on Political Economy, when he objects to early marriages, on the ground that "young girls suffer far more pain, and are more frequently injured in giving birth, because their parts are undeveloped." Ἔτι δὲ ἐν τοῖς τόκοις αἱ νέαι πονοῦσί τε μαλλον καὶ διαφθείρονται πλείοες * * * μικροὶ τα σωμάτα εἰσιν.[2] Whilst Ulpian—a Roman lawyer of the third century who was familiar with the literature of the Greeks—evidently had this passage in mind, when he calls those women "*arctas*," whose openings were small—"*angustis meatibus.*"[3]

It may be objected that these examples are, at best, very doubtful hints which do not conclusively prove a positive knowledge of such lacerations. But, it seems to me that all ancient writers on medicine have alluded to them, if we only take into consideration their confused ideas about the anatomy and terminology of the procreative organs. As Stein observes, "Hippocrates and the ancients had no clear ideas about the openings of the womb, or about those leading to it;"[4] nor had the moderns down to the seventeenth century. They employed such terms as ὑστέρα and μήτρα—in the singular number—ὑστεραι and μήτραι—in the plural number—*matrix, uterus,* and *vulva* indifferently in a generic, as well as a specific sense. Thus, ὑστερα means not only the *womb*, but "*the passage leading to the womb.*"[5] Αι μῆτραι can be rendered either the womb proper, or the entire genital apparatus. Hippocrates introduces a suppository of beef "larger than the big-toe"—ἐς τὰς μήτρας—which Littré[6] translates "into the womb," but which evidently means the *vagina,*[7] and not the womb at all.

[1] De Mulierum Partibus, Ed. 1594, p. 17.

[2] De Republicâ, lib. vii. cap. 16.

[3] Fragmenta, ad Edictum Ædilitium; from Martin Akakia's Medici Regii de Morbis Muliebribus, lib. ii. cap. xiii.

[4] Lehrbuch der Geburtshülfe; Marburg, 1805, par. 167.

[5] Pickering's Greek Lexicon.

[6] De Naturâ Mulierum, § 108.

[7] In view of this confusion in the ancient nomenclature of the female organs of generation, I do not think that Dr. J. Cohntein (Historical Review of Uterine Injections, translated by Dr. Kammerer; Am. Journ. of Obstetrics, 1869, p. 377) has proved his point, in claiming for Hippocrates the priority in the use of intra-uterine injections. Hippocrates, it is true, repeatedly recommends injections for the womb—ἐς τὰς μήτρας (See Littré, vol. viii. p. 189, and sparsim in vols. vii. and viii.); but he also attempts to fumigate the uterine cavity—ἐς τὰς μήτρας—by covering the woman with clothes, and making her sit over a heated vessel into which certain dry herbs had been cast (Littré, vol. viii. p. 111). He also—as we have seen above—introduces "pieces of beef larger than the big-toe—ἐς τὰς μήτρας"—all which conclusively prove that these applications were vaginal, although, perhaps, intended for the womb. Moreover, his method of making these quasi intra-uterine injections shows that they were only vaginal after all. In the third book on Sterility (Littré, vol. viii. p. 431) he describes the syringe, which consisted of a sow's

Galen, who wrote a commentary upon the works of Hippocrates, was so perplexed by this confusion of terms, that he declines to discuss the questions, "Whether the procreating organ, given by nature to woman, should be called *uterus* or *vulva;* and whether the word employed should be used in the singular or the plural number."[1] On the other hand, Pliny states that "the uterus of woman is called *loci*, and of other animals, *vulva*."[2] Both Horace and Martial speak of that delicate and favourite dish of their contemporaries, the *vulva* (womb) of a pregnant sow beaten to death.

By the terms *τὸ στόμα τῶν μητρέων*, *os uteri* and *os vulvæ*, it is often difficult to understand whether the mouth of the womb is meant, or the vaginal entrance. Thus, in the Hippocratean Collection, constant allusions are made to wounds, ulcers, fissures, and ruptures of the *στόμα*; a word which is invariably translated as the *mouth of the womb*, and yet in the treatises on the Nature of Woman,[3] and on Sterility,[4] the *στόμα τῶν μητρεῶν* is said to be occluded sometimes by a membrane (hymen), which he recommends to be cut; showing that the vaginal opening is here meant. Also Celsus—who lived in the first century, and is deemed an elegant and precise writer—calls the *womb* by the word *vulva;* and yet he defines the hymen, as "the membrane occluding the mouth of the womb," *membrana ori vulvæ opposita.*[5] Aristotle, throughout his great work on the Generation of Animals, includes the *vagina* under the generic terms *ὑστέρα* and *μήτρα*, calling the pudendum *το στόμα τῆς μήτρας* (the mouth of the womb). This fact is also noticed by Caspar Bartholinus[6] and by Rodericus à Castro,[7] who both call the vagina by the terms *cervix* or *collum uteri.* The latter, after giving as synonyms the words *μήτρα*, *ὑστέρα*, *matrix*, *vulva*, and uterus, remarks that "certain physicians, for the sake of convenience, divide it [the womb] into the fundus, and into the collum or

bladder attached to a silver tube, closed at the extremity of the nozzle, but pierced with many holes *throughout its length.* "When there is pus"—proceeds Hippocrates—"in the womb (*ἐν τῇσι μήτρῃσι*), preventing conception, it is necessary to inject the womb (*κλύσαι τὰς μήτρας*) with mare's milk," and "this is done by filling the bladder, after first plugging up the holes of the tubes with rags, and by *handing it to the patient herself*, who, after removing these plugs, will introduce it into the womb (*ἐς τὴν μήτρην ἐνθετσθαι*), for *she will know where to place it.*" If this means an *intra*-uterine injection, then classic matrons had longer fingers, and a better knowledge of anatomy than their modern sisters.

[1] De Locis Affectibus, lib. vi. cap. 5. [2] Historia Naturæ, lib. iii. cap. 84.

[3] *Ἡ μύνιγξ ἐπιπρεσθεν τοῦ στοματος γίνεται.* Vander-Linden, cap. lxii. p. 401.

[4] *Μύνιγγα ἐπιπεφυκέναι ἐν τῷ στόματι τῶν μητρέων*, apud Littré, § 223.

[5] De Re Medicâ, lib. vii. cap. 28.

[6] "Collum, cervix sive canalis uteri, Aristotele aliquando etiam *matrix* dicitur, et uteri ostium, Fallopio sinus pudoris." Institut. Anatomic. Lug. Batavorum, 1641, p. 159.

[7] "Sequitur uteri collum sive cervix, ab Aristotele *matrix* etiam nuncupata, quæ masculorum peni correspondet." Universa Muliebrium Morborum Medicina; Colon, Agrippinæ, 1689, Lib. i. p. 7.

cervix;" but that "the neck of the womb (collum matricis) ends at the pudendum, which is called the female genital, the genital mouth, the mouth of the neck of the womb (os cervicis uteri)."[1] Avicenna speaks of the hymen as lying "in ore matricis,"[2] and describes an hypertrophied clitoris as springing up from "the mouth of the womb,"[3] by which term he of course means the ostium vaginæ.

Rufus Ephesus—50 A. D.—evidently includes the whole of the vagina under the generic term *womb*, which he thus defines. "The female genital member, which is called μήτρα, *vulva* and *uterus*, lies between the bladder and rectum, resembling in form a cupping-gourd. *In this place* sexual intercourse takes place."[4] Oribazius, of the fourth century, makes "the uterus extend from its fundus, near the umbilicus, to the pudendum."[5] Aëtius, of the fifth century, the earliest of Christian physicians, estimates "at eleven inches the length of the uterus from the fundus, near the umbilicus to the pudendum;"[6] and so also does Vesalius[7] ten centuries later. Even the word αἰδοῖον had no fixed meaning, but it usually signified "the female genital organs, especially the vagina; in which sense it is used by Galen and Rufus Ephesus."[8] Maurice Corday defines this word as properly the *pudendum*, or "that which is exposed to view when the body is stripped," *quod nudato corpore oculis statim detegitur;* but he admits that, "with Hippocrates, it begins internally at the os uteri and terminates at the pudendum."[9]

By the *cervix* and *collum uteri* the ancients hardly knew what they meant; but generally, in a vague manner, the vagina including its ostium. Jean Lebeaut observes that "the neck of the womb (*col de la matrice*), according to Galen and to the old anatomists, begins from the orifice proper of the body of the womb, and ends at the pudendal opening."[10] This obscurity pervades all subsequent writers. Thus Felix Plater, in his plates of the female organs of generation, calls the pudendum *cervicis uteri os;* his *uterus* includes the whole genital apparatus from the labia to the fundus. By the *collum seu cervix* he means the vagina and peri-

[1] Op. cit., pp. 3 and 9.

[2] Opera Medica à J. P. Morgio et J. Costæo recognita, Venetiis, 1564, Lib. iii. Fen. 21, Tract. i. cap. 1.

[3] Ibid., lib. iii. fen. 21, tract. iv. cap. 23.

[4] De Appelationibus Partium Corporis Humani, lib. ii. cap. 16, from Medicæ Artis Principes, H. Stephanus, Ed. 1567.

[5] "Superiorem ejus partem uterus, hoc est fundum, juxta umbilicum, inferiorem, *ad cunnum* protendit." Medicinalium Collectorum, lib. xxiv. cap. 29, from Ibid.

[6] "Longitudo (matricis), a fundo circa umbilicum *usque pudendum*, est plerumque undecim digitorum." Tetrabib. iv. Sermo iv. cap. i. from Ibid.

[7] De Humani Corporis Fabricâ; Basiliæ, 1543, p. 655.

[8] Adversaria Medico-philologica, from Medico-Chirurg. Review, vol. xlv.

[9] Comment. Primus in Librem Priorem Hippocrat. de Muliebribus, § v.

[10] Maladies des Femmes, Rouen, 1609, vol. ii. pp. 463, 488.

neum.[1] Diemerbroeck defines the *cervix uteri* in the modern way, but adds "*hanc (cervicem) multi cum vaginâ malè confundunt.*"[2] Jacob Ruffius describes the fœtal head as advancing out of the womb "*in exteriorem partem, collum inquam matricis.*"[3] Ambrose Paré speaks of the hymen as the membrane found in opening of the neck of the womb, "*in cervicis uteri orificio membrana reperitur.*"[4] Ludovicus Bonaciolus "plentifully anoints the cervix uteri" *after* the fœtal head has descended out of the womb.[5] Van Swieten, who wrote in 1700, observes that "the vagina is by some called the external neck of the womb."[6] According to Lebeaut, Fallopius was the first anatomist who limited the cervix uteri, "not to that part which receives the virile member, but to the small canal which conducts to the inside of the womb."[7] The unknown French translator of Albertus Magnus makes him say, "It occasionally happens in labour that the womb (la matrice) is torn into the fundament, converting these two openings into one."[8] "It is easy to see," observes Mr. Sue, "that by the 'matrix' the author means the fourchette or perineum."[9] In 1685 Portal blames the haste of midwives "who tear open the neck of the womb" (col de la matrice),[10] meaning thereby the perineum. Even Blundell, so late as 1840, treats of uterine and vaginal lacerations, under the head of "Rupture of the Genitals."[11]

Now, this confusion of terms makes it no easy matter to understand Hippocrates and the other ancient writers. From their works I could multiply examples of *wounds, ulcers, fissures,* and *lacerations* of the μήτρα, ὑστέρα, and στόμα, arising from labour, which Littré and other translators interpret as injuries to the neck of the womb; but which, in the great majority of cases, seem to me to be those of the vagina and perineum.

There is yet another difficulty in the way, and that is the meaning of the word ἕλκος. This noun is invariably rendered by Littré and others, by the word *ulcer.* Yet Foësius contends that ἕλκος and its derivates are often used by Hippocrates and Homer in the sense of a *recent wound* or *laceration.*[12] Again, Daremberg advances the same opinion, asserting that Hippocrates employs this word to describe "a recent wound, or a solution of continuity, arising from either external or internal causes."[13] Finally,

[1] De Mulierum Partibus in I. Spachii Gynæciorum Harmoniâ, Argentinæ, 1597.
[2] Anatomia Corporis Humani, Ultrajecti, 1622, vol. i. 242.
[3] De Partu, etc. in Gynæciorum Harmoniâ, p. 178.
[4] Opera, Paris, 1582, pp. 713, 714.
[5] Enneas Muliebris, cap. viii. in Gynæc. Harmoniâ, p. 141.
[6] Commentaries on Boerhaave, § 1313. [7] Op. cit., p. 463.
[8] Les Admirables Secrets. Lyon, 1791, p. 40.
[9] Essais sur l'Art des Accouchemens. Paris, 1779, vol. ii. p. 28.
[10] Pratique des Accouchemens, from Ibid., p. 229.
[11] Obstetric Medicine. London, 1840, p. 280.
[12] Œconomia Hippocratis, Francofurti, 1588, p. 200.
[13] Hippocrate, Paris, 1843, p. 398.

Hippocrates himself states that "lacerations are most like ulcers,"[1] and this latitude of interpretation clears up many ambiguous passages in the collection bearing his name. For instance, a treatment is given "when the mouth of the vagina is ulcerated [*torn*] by the violent birth of the child;"[2] "when ulcers [*lacerations*] of the opening arise from labour;"[3] "when the pudenda become *ulcerated* from labour;"[4] "when *ulcerations* arise from an abnormal delivery of an infant."[5] So numerous, indeed, are these allusions to injuries of the vulva, that, according to Mercatus, "Hippocrates speaks of their causes a thousand times."[6]

Lacerations of the perineum were therefore not unknown to Hippocrates and to his immediate disciples; but let us pursue the inquiry further, bearing in mind the confused terminology of ancient anatomy. 50 B. C. Cleopatra recommended an astringent treatment "when the *matrix* is wounded from difficult labour."[7] Celsus—who lived in the first century—is represented by I. Baker Brown[8] as describing these lacerations and treating them like ordinary wounds; but I can find no passage in his works bearing on such injuries, other than one on the operation of embryulcia, in which he cautions the physician not to let the crotchet slip and tear the "vulva" (womb).[9] About the year 100 A. D., Archigenes wrote on lacerations and erosions of the womb and vagina; in the latter, he adds, "they far more frequently occur." He also gives the formula of an ointment, "when the pudenda are *exulcerated* by a difficult labour, or by a forced delivery."[10] Soranus—his contemporary—in a treatise on the female organs of generation, speaks of women "whose vagina is short and deformed."[11] Galen—150 A. D.—besides editing the works of Hippocrates, recommends a plaster composed of bulls' testes, saffron, and roses, "whenever through labour the pudendum is torn"—si ex partu *natura* plagata fuerit.[12] He also enumerates among the causes of ulcers in the maternal passages, "difficult birth and artificial extraction of the infant."[13] Moschion—who lived in the second century, and is the earliest writer who describes natural labour—advises a cerate "for the lacerations which so fre-

[1] Ἐγγυτάτω δὲ ἕλκεός ἐστι τὸ ῥῆγμα. De Morbis, apud Vander-Linden, lib. i. cap. xvi.

[2] Ἢν ἑλκωθῇ τὸ στόμα τῦ αἰδοίου, καὶ ἐπιδὰν ἑλκωθῇ ἐν τόκῳ βιηθεν ὑπὸ τῆς ἐξοδου του παιδιου. De Morbis Mulierum apud ibid. Lib. i. cap. 68.

[3] De Mulierum Morbis, apud Litrê, lib. i. cap. 49. [4] Ibid., cap. 64.

[5] Ibid., cap. 67. [6] De Mulierum Affectionibus, Lib. ii. cap. 20.

[7] De Passionibus Matricis; in Spachii Harmoniâ, p. 37.

[8] Surgical Diseases of Women, p. 13.

[9] De Re Medicâ, lib. vii. cap. 29.

[10] Aëtius, tetrabib. iv., sermo iv. cap. 65, 88.

[11] Oribazius; Medicinal. Collect., lib. xxiv. cap. 32.

[12] Hippocratis et Galeni Opera; Renatus Charterius, Parisiis, 1679, vol. vii. p. 874.

[13] De Humoribus, in Spacchi Harm., p. 957.

quently result from the throes and stretchings of labour."[1] Aëtius, of the fifth century, cautions the midwife, in the last stage of labour, "not to lacerate the *uterus*, but to hasten to put the woman on the obstetric stool." He also gives a treatment "for the fissures of the mouth of the womb, which arise in violent labour from the great size of the child's head."[2] Unfortunately the special treatise on the diseases of women, by Paulus Ægineta, of the seventh century, is lost; but his seven books on medicine contain many allusions to lacerations, ulcerations, and fissures of the *womb* "produced by violent labour." He also seems to hint at lacerations of the perineum in the following sentence: "Procidentia of the womb also arises from a *bungling* operation of embryulcia."[3] In the eighth century we find Theodore Priscianus recommending Galen's ointment for lacerations of the perineum, and to which he adds a little olive oil."[4] The Arabian physicians Avicenna and Albucasis, who flourished respectively in the tenth and eleventh centuries, frequently allude to fissures, lacerations, wounds and ulcers of the womb and of the genital passage, arising from labour, and give minute directions for their treatment. The former gives a method of treatment for the "*apostemata calida* of the mouth, body-cavity, sides, and of any portion of the *womb*," one of whose causes is "laceration made by the midwife when she delivered the infant"—*causa est . . . disruptio ex obstetrice, quum recepit fœtum.*[5] Avicenna, as we have already seen, often employed the term *uterus* in a vague and generic sense, and here he evidently refers to laceration of the perineum.

We next come to a fragment entitled "De Passionibus Mulierum," which describes complete laceration of the recto-vaginal septum and its treatment, in the following language: "Sunt quædam quæ ex gravitate partûs incidunt in rupturam pudendorum . . . Item quibusdam mulieribus in pariendo malé accidit, propter defectum assistentium. Sunt enim quædam quibus vulva et anus fiunt unum foramen . . . Postmodum rupturam intra anum et vulvam tribus locis vel quator suimus cum filo de serico. . . . Deinde pice liquidâ linimus; hoc enim facit matricem retrahi, propter sui fœtorem."[6] This interesting fragment contains probably the earliest authentic mention of a complete laceration of the perineum, and is also the first to give a method for both its cure and prevention. Unfortunately, neither the name nor the time of the author is known. The learned Gessner[7] deems this fragment to be the composition of Eros, a freedman of

[1] "Εἰ δὲ ἐκ τῆς ὁρμῆς καὶ τάσεως συνεχέστερον ἐν τῷ τόκῳ τὰ ῥήγματα συμβαίνουσι." Περὶ Τῶν Γυναικείων Παθῶν, Κεφ. 57.

[2] Tetrabibli iv. sermo. iv. cap. 22, 107.

[3] De Re Medicâ, lib. iii. cap. 72, 75.

[4] In I. Spacchii Harmoniâ, p. 14.

[5] Op. cit., lib. iii. Fen. 21, Tract. iv. cap. 10.

[6] De Passionibus Mulierum in Spachii Harmoniâ, p. 50.

[7] Histoire Sommaire de l'Art d'Accoucheur. Astruc, 1766, p. 78.

Julia, the debauched daughter of Augustus Cæsar, A. D. 10. Astruc,[1] Denman,[2] and Velpeau attribute it to Eros,[3] surnamed Trottula, a midwife of the school of Salerno, A. D. 1250; whilst Renzi[4] and Daremberg[5] place her in the year 1059 A. D. On the other hand, Sir Alexander Croke says "Trotta, or Trottula, is allowed to be a forgery;"[6] an opinion confirmed by Sprengel,[7] Portal,[8] and Gruner,[9] who assert that this fragment is neither by Eros, nor by Trottula, nor by any pagan physician, but by some Christian physician of Salerno.

About the year 1240, at the solicitation of an inquisitive priest, Albertus Magnus, the Bishop of Ratisbon, wrote a very unepiscopal work on the "Secrets of Women." It contains much prurient matter, which will hardly bear translation, and yet was deemed worthy of a commentary by so devout an ecclesiastic as St. Thomas Aquinas.[10] Besides an *olla-podrida* of falconry, astrology, nostrums, and filth, this remarkable book contains a short chapter on parturition, in which the author states that "sometimes it happens in the labour of woman that the *vulva* is torn into the anus, converting these two openings into one;"[11] but I am sorry to say that this "Philosophus Philosophorum Princeps"—as he is called in the preface—gives no method of avoiding this accident.

That there were other writers upon this accident, previous to the sixteenth century, whose works I have not been able to reach, is evident from the following passage taken from the works of Ambrose Paré: "The ancients have shown (veteres prodiderunt) that, in violent labour, the maternal genitals may be so torn that the anus and vulva are converted into one opening."[12] He, however, advises no preventive measures other than the use of lubricants. From the time of Paré to the middle of the

[1] Ibid. [2] Loc. cit. [3] Loc. cit.

[4] Collectio Salernitana. Naples, 1852, vol. ii. p. 415.

[5] La Médecine, Histoire et Doctrines. Paris, 1865, p. 140.

[6] Regimen Sanitatis Salernitanum. Oxford, 1830, p 14.

[7] Histoire de la Medicine. Paris, 1815, vol. ii. p. 361.

[8] Histoire de l'Anatomie. Paris, 1770–1773.

[9] Dictionary of Biog. and Myth., by W. Smith, LL. D.

[10] Medical Bibliography, by James Atkinson, London, 1834.

[11] "Accidit etiam in partu mulieris, quod rumpitur *vulva* usque anum, itaquod illa duo foramina unum fiunt." De Secretis Mulierum; Argentorati, 1537, and also Lugduni, 1596. It is uncertain what edition was translated into French (see page 58); but, in the two editions from which this quotation is taken, there is a sharp distinction drawn between the meanings of the terms "*vulva*" and "*matrix*." For, further on, it is stated that this accident to the *vulva* is attended with a prolapse of the *matrix*. I am inclined to think that Eros and Albertus Magnus are among the earliest writers who use the word *vulva* in its modern sense. *Vulva*—the womb—is classical Latin; but *vulva*—the pudendum muliebre—is ecclesiastical or medieval Latin: a sufficient proof that Eros cannot be of great antiquity; at least not of the Augustan age, as Gessner contends.

[12] Opera, lib. xxiii. cap. 27.

last century, many obstetric writers—by no means all—speak of this accident; but, apart from artificial dilatation and lubricants, I can find only three who give any method of preventing it; and these three physicians—as we shall presently see—adopted the plan of Eros, that of plugging up the anus. Thus Hieronimus Mercurialis,[1] 1550 A. D., cautions against lacerating the perineum, and yet advises nothing but the obstetric stool. Jean Le Bon wrote his book, as we learn from the preface, "*dilaniari enim non paucas gravides mulieres a chirurgis, obstetricibus et tonsoribus . . . sæpius quàm voluissemus, vidimus;*" indeed, he devotes one chapter to the subject of perineal lacerations, and yet recommends no preventive measures but forced dilatation, and the application of the oils of white lilies, almonds, or of linseed.[2] Nicholas Culpepper twice warns his readers that "the Perineum sometimes breaks with the skin from the Privities to the Arsehole;"[3] but he seeks to avoid this accident simply by making his patient swallow white amber, or lily water, and by inunctions with the oil of lilies, or with the mucilage of althæa. Jacob Primrose[4] and Rodericus à Castro[5] both recognize this danger, but give no method of support. The latter objects to the use of sutures in closing such lacerations, on the ground that the cicatrix will be torn in a succeeding labour. On the other hand, among a crowd of writers, Eucharius Rhodion,[6] Thomas Raynalde,[7] Jean Liebeaut,[8] and that celebrated midwife, Louise Bourgeois,[9] make no allusion whatever to these lacerations; but dilate the parts with their fingers and apply the oil of white lilies, which appeared to be the great emollient of that day.

From these facts it is clear that the arguments of Denman and Leishman fall to the ground, because: 1. Lacerations of the perineum are alluded to by most of the ancient writers. 2. The Christian religion had nothing to do with the preservation of the perineum; since pagan and Mohammedan polygamists both describe and seek to cure its injury, whilst Aëtius, the earliest Christian physician, merely hints at it, and gives no remedy whatever. 3. The proportion of the ancient and of the "modern ancient" obstetric writers, who omit to notice these lacerations, is about the same; whilst, until the middle of the last century, I can find but *four* who describe any systematic plan to prevent them.

It appears to me that two valid reasons can be given why none of the

[1] De Morbis Mulierum, lib. iv. cap. 7.
[2] Therapia Puerperarum, part. 1, 4, in Spacchii Harmoniâ.
[3] A Directory for Midwives. London, 1671, p. 178.
[4] De Mulierum Morbis. Roterodami, 1655.
[5] Universa Mulieb. Morb. Medicina, 1689, lib. iv. p. 442.
[6] De Partu Hominis, Ed. 1522, chap. iii.
[7] The Birthe of Mankinde. Ed., 1598, chap. iii.
[8] Maladies des Femmes. Rouen, 1609, pp. 881, 882.
[9] Observations Diverses. Ed., 1617, vol. ii. p. 153.

older writers lay down any positive rules of guidance for the management of the perineum, and why, comparatively speaking, so few of them describe its laceration with any degree of clearness.

(*a*). The use of the obstetric chair or stool, which was universal, was supposed in itself to protect the perineum, by keeping the child's head in the axis of the pelvic canal, but it also placed the woman in such a position as to preclude any manual assistance. (*b*). The general practice of midwifery being exclusively in the hands of midwives, physicians were not called in unless special difficulties arose, and they were therefore kept in ignorance of the more normal processes of parturition. This is evident from the fact that Hippocrates, Galen, Celsus, and the older writers treat only of dystocia; whilst Moschion, who was the first to describe natural labour, is also the first who writes, like an eye-witness, upon perineal lacerations. "Do not refuse," says Hippocrates, "to believe women on matters concerning parturition."[1] "It is unnecessary," writes Aëtius, "to give a treatise on midwifery, because, from long experience, not only do midwives, but also all other women know this subject perfectly."[2] "I am informed *by midwives*," explains Albertus Magnus, "that when the head presents, all goes well; but when an arm or a foot, then danger arises." This sentence clearly shows whence he obtained his information about complete perineal lacerations, and why he gives no preventive treatment. How else is it that, on the one hand, the credulous Heister[3] tells us of a clergyman's wife who coughed her perineum open; and on the other, Fabricius ab Aquapendente doubts whether, from labour, "the whole perineum from the anus to the pudenda may be dilated and ulcerated"—"laxari et ulcerari"[4]? How can we explain the inconsistency of Hippocrates, who compares the fœtus in the womb to an olive in a bottle, which can only be withdrawn by one or the other extremity, and yet asserts that a pelvic presentation is generally fatal to both mother and child,[5] unless the only breech cases which he saw were those in which the head was arrested by a contracted brim, and he was called in by the midwives to perform craniotomy? How otherwise can we account for the perpetuation of this error until the seventh century, and for its ultimate refutation by the first practical accoucheur of antiquity—Paul of Ægina, surnamed Obstetricus?

After showing that the ancients were familiar with lacerations of the perineum, it remains to inquire into their methods for avoiding this accident. Down to the middle of the last century I cannot find a single proof that the perineum was ever supported by the hand. The means employed were delivery on the obstetric stool, or on the hands and knees when the patient was fat; the warm bath; free inunction and arti-

[1] Littré, vol. vii. p. 441.
[2] Tetrabibli iv. sermo iv. cap. 14.
[3] Medico-Chirurgical Observations, No. 614.
[4] Medicina Practica, Parisiis, 1634, p. 709.
[5] Littré, vol. viii. p. 79.

ficial dilatation of the external parts. Internal remedies were also exhibited, such as warm olive oil, the oil of lilies, the infusion of saffron, of swallows' nests, and especially of sage leaves, which was in high repute, because, according to Mercatus, it was a well-known fact that lionesses chewed this plant in order to have easy labours.[1]

Before the disuse of the obstetric stool the harmony of the profession with reference to the management of the perineum was edifying. But when women began to be delivered on their own beds, and—suspicious coincidence—shortly after the discovery of the forceps, the liveliest discord arose, as we shall soon see. About the middle of the last century the pioneers in the revival of midwifery were impressed with the idea that, in the process of parturition, the perineum had been overlooked, and that art in some shape must come to the rescue. In 1783 Vander Eem gravely congratulates the moderns upon the discovery of "perineal support, neglecting which the ancients must have experienced frequent lacerations."[2] Both Gardien[3] and Filhastre[4] assert that Puzos was the *first writer* who suggested *manual support* to the perineum, and my own independent researches confirm this statement. In his work on midwifery, written in 1747 and published posthumously in 1759, Puzos advocates support to the perineum from behind forwards towards the pubis. At the same time, however, he confesses that partial lacerations are *inevitable* in first labours, complete through the sphincter ani about once in a thousand labours, and that this support is not, after all, worth much, "since nature does all in the last pain;" hence he relies rather on moderating the expulsive pains, and on pushing the adjacent skin towards the perineum.[5] That this method of support did not become general in France until adopted by such men as Levret[6] and the elder Baudelocque is evident from Astruc, who is a valuable witness in this inquiry, because, being no obstetrician, he rode no hobby. In the preface to his book—L'Art d'Accoucheur—compiled in 1745 and first published in 1766, he confesses never to have delivered a woman, and that his treatise is a mere compendium of all the known works on midwifery, but especially of those authors who wrote within the thirty preceding years. Yet he nowhere refers to the necessity of supporting the perineum, although he frequently alludes to the injuries it receives in labour, and seeks to avoid them by artificial dilatation and by pushing the coccyx backwards.

In England the first incidental mention of perineal support is by Gif-

[1] De Mulier, Affect., lib. iv. cap. iii.
[2] Schlegel's Sylloge Operum Minorum, vol. i. p. 76.
[3] Traité Complêt d'Accouch., 1824, vol. iii. p. 287.
[4] Essai sur la Situation de la Femme, 1816, p. 24.
[5] Traité des Accouch. 1759, pp. 115, 125, 129.
[6] Tolver's Present State of Midwifery in Paris. London, 1770, p. 9.

ford[1] in 1734, and next by Smellie[2] in 1764. In 1767 John Harvie advocated "support" as a novelty in his "Practical Directions, showing a method of preserving the Perineum at Birth." Three years later Tolver showed that this was the practice in Paris (loc. cit.). But it was only through the influence of Hamilton[3] in1775, of Foster[4] in 1781, and of Dease[5] in 1783, that "support to the perineum" became an established canon in midwifery. In 1784 Hamilton[6] published another edition of his work, from which it appears he had greatly changed his views with regard to the value of "support," for he terms it "a doubtful and hazardous expedient." Osborne,[7] in 1792, revived the plan with much zeal; whilst Bland,[8] in 1794, takes Osborne very severely to task, and scornfully speaks of "guarding the perineum" as "the method which has been *lately added.*"

Among the Germans, Osiander and Mohrenheim had, it is true, anticipated Stein[9] by a few years; but, in 1785, the latter was the fugleman of the partisans of "support." Thus, by the influence of a few enthusiasts, one of whom subsequently recanted, this error spread so rapidly over the whole civilized world, that, at the present day, "support the perineum' has become the watchword of all the schools of midwifery. To question its utility may seem presumptuous, but in the issue, I am emboldened by the confused and conflicting counsels of its advocates, a sure evidence of a weak cause. Let us see, then, what methods for avoiding laceration of the perineum have been recommended by the most eminent obstetricians, and their reasons for so doing.

Eros advised that "an oblong cloth tent be placed in the anus, which is to be firmly pressed upon during every expulsive pain."[10] This method was adopted by Louis de Mercado[11] in 1580, by D. Sennertus[12] in 1632, and by Melchior Sebizius[13] in 1672, but seems, after this last date, to have fallen into disuse.

All the classic writers upon midwifery, and many of their followers, even such men as Smellie and Baudelocque, artificially dilated the external

[1] Cases in Midwifery. Case clxiv. [2] Collection XL., Case 2.
[3] Elements of the Practice of Midwifery, p. 138.
[4] Principles and Practice of Midwifery. London, p. 141.
[5] Observations in Midwifery. Dublin, p. 31.
[6] Outlines of the Theory and Practice of Midwifery. Edinb. p. 210.
[7] Essay on the Practice of Midwifery, London.
[8] Observations on Human and Comparative Parturition; Preface of.
[9] Lehrbuch der Geburtshülfe. Marberg, 1805, § 688.
[10] Ad prædictum igitur periculum (rupturam pudendorum) evitandum, eis in partu dilligentissimé sic providendum est. Præparetur pannus in modum pilæ oblongæ, et ponatur in ano, ad hoc ut in quolibet conatu ejiciendi puerum, illud firmiter ano imprimatur, ne fiat hujusmodi continuitatis solutio. De Passionibus Mulier.
[11] De Mulier, Affectionibus, lib. iv. cap. 3. [12] Practicæ Medicinæ, lib. iv. p. 7.
[13] Schlegel's Sylloge Operum Minor, vol. i. p. 76.

opening with their fingers; Pugh "stretched lengthwise when the pains are off, and circularly when the pains are on." At the present day under no circumstances whatever is the perineum thus treated.

For many centuries it was customary to drag out the child's head by the ears, in order thus to relieve the parts from too great pressure. But both M. Gardien[1] and Madame le Boursier du Coudry[2] object to this method; the former on philosophic grounds, the latter because she had known the ears to be pulled off.

Stein, and his numerous disciples were accustomed to scoop out the head by two fingers passed in well over the forehead "in the form of a shoe-horn," as Mackintosh[3] derisively termed it. Whilst Roonhuysen and Gehler[4] used the vectis for the same purpose.

Others reversed this process, and hooked down the fourchette by the two forefingers of one hand, over which the fœtal head glided. Such men as Deleurye,[5] Petit,[6] and Mesnard[7] were advocates of this measure; whilst Fried and Roederer improved upon this by using the two forefingers of each hand conjointly. Gardien[8] justly remarks that this was a very unscientific method, because the perineum—being stretched still more by the presence of two or four fingers—could not adapt itself to the circumference of the child's head.

Gehler,[9] Smellie, Leishman,[10] Danz,[11] Hagen,[12] Nedel,[13] Rickets,[14] and the irrepressible Stein—when he could do nothing else—recommended that the head be guided to the pubic arch by two fingers inserted into the rectum. On the other hand, one or more fingers were passed into the rectum, to force back the coccyx, by Astruc,[15] Willoughby,[16] Le Bas,[17] Ould,[18] and by many other distinguished physicians. Van Doeveren,[19] however, objected to

[1] Traité d'Accouch., vol. iii. p. 288.
[2] Abrégé de l'Art des Accouch. Paris, 1759, p. 69.
[3] Blundell's Principles and Practice of Midwifery, 1840, p. 111.
[4] Ryan's Manual of Midwifery, art. vi. sec. v.
[5] Traité des Accouch. 1777, p. 265.
[6] Mecanisme de l'Accouch., vol. i. p. 298.
[7] Le Guide des Accouch. 1753, p. 145.
[8] Traité des Accouch. 1824, vol. ii. p. 286.
[9] Programma de Ruptura Perinæi. Leipsic, 1781.
[10] Mechanism of Parturition, p. 99.
[11] Starks N. Archiv. sür die Geburtshülfe, B. iii. p. 631.
[12] Journal de Médecine, tom. xxxvii. p. 309.
[13] Starks N. Archiv. etc., B. i. p. 406.
[14] Medical Times and Gazette, vol. i., 1863, p. 415.
[15] L'Art d'Accoucher, 1766, pp. 175, 270.
[16] Essais Hist. des Accouch. par Sue, vol. ii. p. 249.
[17] L'Art d'Accoucher, 1780, p. 58.
[18] Leishman's Mechanism of Labour, p. 88.
[19] Schlegel's Sylloge, vol. i. p. 75.

this method, because he had seen the gut thereby injured, and a diarrhœa induced. Daventer[1] also opposes it, but on the ground that the force exerted by two fingers is too feeble, and displaces the tip only of the coccyx; he therefore passed his entire hand into the vagina, and pressed firmly, with the palm, upon all the bones of the coccyx. "I can take my oath," he says, "that this operation has never torn, nor sensibly injured either the womb, its mouth, the vagina, or rectum."

Jacobs,[2] of Gand, Mauriceau,[3] and his translator, Hugh Chamberlen in 1752, Giffard,[4] and Hamilton[5]—in the earliest edition of his work on midwifery, but not in the later—recommend the perineum to be pushed backwards over the presenting part towards the anus. Plenck says: "Push back the perineum as if to make it enter the anus."[6] Madame le Boursier du Coudray (loc. cit.) separates the labia, and pushes them backwards. On the contrary, Churchill lays down the sweeping rule to "draw the integuments forwards, but never backwards."[7] To gain more skin, La Tourette,[8] T. Snow Beck,[9] and Schaffler[10] press the perineum forward. With the same object in view the skin of the thighs is rubbed upwards by Wigand,[11] Von Siebold,[12] and Braun,[13] whilst Puzos[14] and Flamant,[15] with both hands, pushed the nates towards the vulva.

But, despite all these ingenious devices, the perineum would tear; Dionis,[16] therefore, forcibly dilated it, and slid his two hands between the labia and the child's head. With more foresight, Johnson[17] and Bonnet[18] strengthened the edge of the vulva by a ring, made by the meeting of the two thumbs posteriorly, and of the forefingers anteriorly. Hohl,[19] Busch,[20] Braun, W. Tyler Smith, and Wilson,[21] of Glasgow, counsel direct pressure on the child's head to restrain or guide it. "Osborne[22] "strenuously resists the progress of the head with one hand, and firmly supports the perineum with the other." Blundell delays delivery "by pressing very

[1] Operat. Chirurg. Novum Lumen, etc., 1701, chap. 27.
[2] Ecole Pratique des Accouch., vol. i. p. 115.
[3] Maladies et Grossesse des Femmes, 1668, lib. ii. cap. 8.
[4] Cases in Midwifery. London, 1734.
[5] Elements of the Prac. of Midwif. London, 1775, p. 138.
[6] Elémens de l'Art des Accouch. Lyons, 1792, p. 61.
[7] System of Midwifery, Am. ed. p. 465.
[8] L'Art des Accouch. 1785, p. 109.
[9] British Med. Journ. March, 1860, p. 197.
[10] Hufeland's Journ. der pract. Heilkunde, B. 13, p. 177.
[11] D. H. Agnew on Lacerations of the Female Perineum, p. 8. [12] Ibid.
[13] Glasgow Med. Journ., January, 1860. [14] Op. cit., p. 132.
[15] Velpeau's Traité d'Accouch., vol. ii. p. 38.
[16] Traité Général des Accouch. 1724, p. 218.
[17] A New System of Midwif. Lond. 1769, p. 198. [18] Cours d'Accouch., p. 417.
[19] and [20] D. H. Agnew. [21] British Med. Journ. 1861, vol. i. p. 654.
[22] Essays on the Practice of Midwif. 1792, p. 36.

hard against the head with both my hands."[1] Ramsbotham[2] braced his elbow against the bedstead, regarding it a fixed point, and allowed the perineum to be forced against his hand, in order to restrain the advance of the head. In this fatiguing position this unhappy man confesses to have remained "for hours." White pressed against the perineum with such force as "to prevent a too hasty delivery."[3] So did Baudelocque[4] and Ryan.[5] Capuron[6] used support to favour extension of the head; Chevreuil[7] and Maygrier[8] to counterbalance the pressure of the head. Mayor (loc. cit.), when necessary, used the hands of an assistant in addition to his own.

On the other hand, Churchill says: "I must altogether object to any attempt to retard the passage of the child, as erroneous in theory and mischievous in practice;"[9] and Velpeau, that "the object of support is to compel the head to extend and hug the pubis, and not to hinder its descent."[10]

Again; the advocates of "support" differ much as to the best manner of making it. Gooch[11] applied "the hand to the thin attenuated edge to form an artificial perineum." Wigand[12] laid his "well-chalked" thumb across the fourchette. Nedel[13] and Clay,[14] of Manchester, also use their thumbs, but without the chalk. Niemeyer[15] sustains the perineum in the *hollow* of the hand, leaving free one inch back of the posterior commissure. Smellie clapped the *flat* of his hand against the *whole* perineum (*loc. cit.*). Meigs, the two Ramsbothams, Osiander, and Mohrenheim,[16] Pretty[17] and Bedford, recommend the application of a folded napkin to the perineum. The Irish physicians,[18] together with Velpeau and his followers, wrap the napkin around the hand. Whilst Stark and Stein (*loc. cit.*), Hodge, Cazeaux, and Blundell scout the napkin, and use the palmar surface of the naked hand; some transversely, and some longitudinally on the whole perineum. Clay uses this strong language: "The common practice of supporting the perineum with a napkin is most absurd; first,

[1] Op. cit., p. 111.
[2] Principles and Practice of Obstetric Med., etc., Am. ed. 1865, p. 151.
[3] Treatise on the Management of Pregnant and Lying-in Women. London, 1777, pp. 104 and 107.
[4] L'Art des Accouch., § 651.
[5] Manual of Midwifery, art. vi. sec. 5.
[6] Cours d'Accouch. 1823, p. 258.
[7] Précis de l'Art des Accouch., p. 82.
[8] Elémens des Accouch., vol. i. p. 229.
[9] System of Midwifery. Am. ed. 1855, p. 465.
[10] Traité des Accouch., vol. ii. p. 37.
[11] Practical Compendium, p. 150.
[12] Ploucquet's Litteratura Medica.
[13] D. Hayes Agnew, op. cit.
[14] Handibook of Obstetrical Surgery. Lond. 1856, p. 147.
[15] D. Hayes Agnew, op. cit.
[16] Stein's Lehrbuch der Geburt, § 677.
[17] Aids during Labour, 1856.
[18] Dublin Practice, p. 98.

because the pressure is not equal, and the accoucheur has no means of knowing where the pressure is most wanted; and lastly, which is of immense importance, it absorbs all the natural mucus, and thus increases the tendency to laceration, an accident in midwifery by no means rare."[1] Chailly Honoré,[2] and Braun,[3] standing behind the patient, pass the left hand from above between the thighs to press upon the head, whilst the perineum is guarded by the right hand. Samuel Smith and the two William Heys—we cheerfully give their names in full—press upon the perineum with the clenched fist.[4] Barlow[5] supports the perineum until it is well distended, and then lets nature do the rest.

Velpeau, Mayor,[6] Churchill, Simpson, Jacquemier, Burns, Dewees, and a host of other well-known authors, advocate direct pressure from behind forwards to favour extension of the head. But Murphy[7] and Niemeyer make it in the axis of the inferior strait to prevent this very extension; whilst W. Tyler Smith avoids touching the perineum, and simply retards the head. On the other hand, Graily Hewitt[8] contends that the head cannot thus be retarded, and that no amount of external pressure can facilitate the process of extension.

Chailly Honoré (*loc. cit.*), Kilian,[9] Rigby,[10] Jackson,[11] and Duparcque[12] deem the child's shoulders to be the most frequent cause of injury to the perineum, and therefore advise firm counterpressure during the process of their delivery. Hoefft,[13] of St. Petersburg, on the contrary, supports only during the passage of the head and leaves the shoulders to nature. Since adopting this plan, he declares that he meets with fewer lacerations in one year than he formerly did in one month.

Murphy advocates support, "first, to allay irritation, and to diminish congestion, so that the act of dilatation may not be interfered with. Secondly, to counteract the too violent action of the uterus." But Leishman, Hewitt, and W. Tyler Smith well object that this very support will not only increase irritation and congestion, but also will interfere with dilatation, and, by reflex action, create violent expulsive efforts.

To sum up, then: there are those who make pressure upon the perineum to retard the head; those who make pressure to accelerate its advance; those who deny that any such effects can be thus produced; and those who conscientiously use support because something must be done.

[1] Op. cit., pp. 146, 196.
[2] Accouchemens, p. 345.
[3] Glasgow Med. Journ., 1859.
[4] Brit. Med. Journ., 1860, p 157.
[5] D. Hayes Agnew.
[6] Instructions sur l'Art des Accouch. Lausanne, 1828, p. 166.
[7] Brit. Med. Journ., 1861, p. 426.
[8] Ibid., 1860.
[9] Die Operat. Geburtshülfe, p. 171.
[10] System of Midwifery in Library of Med., vol. vi. p. 112.
[11] Brit. Med. Journ., 1860, p. 233.
[12] Hist. Complète des Ruptures, etc. Paris, 1863, p. 410.
[13] Jacquemier's Accouch., vol. i. p. 637.

Again; there are those who direct all the pressure at the fourchette; others who reprehend this, and as carefully guard the posterior perineum; and yet others who will not touch the perineum on any account. Further, there are those who push the perineum backwards; and those who, for equally plausible reasons, push it forwards. Some dilate the sphincter vaginæ; some the sphincter ani; and some who plug it up. Some place their hands transversely across the perineum; some longitudinally, with the fingers looking upwards; some longitudinally, with the fingers looking downwards; some who attack it with their knuckles. Some scoop out the head with the vectis; others drag it out by the ears; and yet others who rely on the forceps. Finally, there are those who use the right hand, and those who swear by the left hand. Some who advocate a folded napkin; some an unfolded napkin; and others again who frown down upon all napkins, folded or unfolded.

Wonderful, wonderful, and yet again wonderful diversities of opinion! So this is that "support," on possessing which Vander Eem congratulates the moderns!

> Non tali auxilio, nec defensoribus istis
> *Perinæum* eget.

This is disheartening enough; but, alas! we have not yet done. When there are congenital peculiarities complicating the mechanism of labour; when the perineum is rigid; the presenting part of the child's body too large, or in an unfavourable position; when from any cause laceration seems imminent, again commences another clash of opinion.

Michaelis[1] advises incision at the posterior commissure; Siebold[2] and Ritgen,[3] where the tension is least, and the head presses the least. Penrose, Eichelbury, Dubois, Blundell, Barnes, Hewitt, Butignot,[4] and Chailly Honoré make lateral or oblique incisions. Simpson nicks the sides of the vulva with his finger nails; whilst Scanzoni and Clay do the same thing with a blunt instrument. Cohen[5] cuts subcutaneously through from one-third to one-half of the fibres of the constrictor muscle near the clitoris. On the other hand, Wallace, Moreau, Busch, Hodge, Hütter, and a host of other authors, object to the use of the knife, and depend upon either the forceps or upon such measures as are usually adopted for relaxing undue rigidity of the maternal soft parts.[6]

Does not this discordance in the rank and file of the profession show a lame cause? Granting that the perineum requires support—and this seems to be the opinion of the vast majority—is it not strange that there

[1] Cazeaux's Midwifery, 4th Am. ed., p. 588.

[2] Bulletin de Ferussac, tome ix. [3] Ibid., tome xvi.

[4] Gazette des Hôpitaux, 1859, p. 70.

[5] Mon. f. Geburt., supplement of 1861, p. 106.

[6] Much of this information I have gleaned from that most admirable pamphlet on Lacerations of the Female Perineum, by Prof. D. Hayes Agnew.

has not yet been devised one single method of giving this support, which by meeting every requirement has commended itself to the entire profession?

Is it not marvellous, that in the management of the only stage of labour which appeals to more than one of the five senses of the physician—those of sight and touch—there is a greater diversity of opinion than in that of any other stage? Is not this fact alone a strong argument that the perineum was made to take care of itself, and not to be supported? "We cannot"—writes Seneca—"complain of the malignity of nature."[1] "Am I to believe"—asks Leishman—"that nature, after making such admirable provision for the earlier stages of labour, bungles matters to such an extent at the end, as to render the aid of the obstetrician in every case necessary to remedy a mechanical deficiency!"[2]

When one sees, for the first time, the maternal soft parts stretched out to a diaphanous thinness by the presenting portion of the child, to all appearances just upon the point of cracking open, the impulse to place the hand upon the bulging flesh becomes almost an instinct. We must not, however, forget that these tissues are not only elastic, but living and sentient; and—what is of still greater weight—that the process of labour is a strictly physiological act. Nature in all her operations intends to adapt means to ends, and the perineum was certainly not created to be torn, unless shored up by the hand of the physician. It unfortunately labours under the disadvantage of lying on the external surface of the body, and its distension is really the only visible process of parturition. Were the os uteri or the uterus itself equally visible and tangible, I doubt not we would be frightened into encircling them with our hands, greasing them, folding towels about them, etc. etc. Fortunately for their integrity they lie out of sight and out of reach.

Another argument in favour of letting the perineum alone as much as possible lies in the fact that those who do so have at least no reason to change their course. Bland believes any support unnecessary "provided there were no improper interference in the early part of labour."[3] Nägele declares that "under ordinary circumstances any support to the perineum is unnecessary."[4] His fellow countrymen Faust, Wigand,[5] Schmidt,[6] Mende,[7] Ritgen,[8] and Kilian,[9] are entirely opposed to any support. In France, Grand-Champ,[10] Danyau,[11] Sacombe,[12] and Deleurye[13] partake of the

[1] De Beneficiis, lib. vii. cap. 1.
[2] Mechanism of Labour, p. 93.
[3] Observations on Human and ComparativeParturition, 1794, p. 60.
[4] Glasgow Med. Journ., vol. vii. p. 413.
[5] D. Hayes Agnew, etc.
[6] Bullet. de. Ferussac, vol. ix. p. 262.
[7] Velpeau's Traité d'Accouch., vol. ii. p. 36.
[8] Ibid.
[9] Die Operat. Geburtshülfe, p. 167.
[10] Thèse, No. 13. Paris, 1825.
[11] D. Hayes Agnew.
[12] Le Médecin-Accoucheur. Paris, 1791.
[13] Traité des Accouch., 1777, p. 265.

same opinion. In England, West[1] of Alford, Swayne[2] of Bristol, Graily Hewitt,[3] Taylor, Thompson, and Leishman have long since discarded support to the perineum, not only as being useless, but as positively injurious. Prof. Taylor of Bellevue declares "that support of the perineum is unnecessary, and that as many lacerations occur in the practice of those who support the perineum as in those who do not." Swayne (loc. cit.) has for eighteen years left the perineum to nature, and his father for forty years before him. The former remarks: "I cannot say that I have not met with cases of lacerated perineum, but they have been very few, and in none has the sphincter ani been involved." Graily Hewitt, who denies that perineal support directs the head forward; denies that it retards or accelerates delivery; denies that it relieves irritation; denies, in one word, all that is, or has been, or ever will be affirmed of support by its advocates, trusts either to nature or to his forceps.

A third argument against manual interference is the admission of its champions, that an unsupported perineum is not necessarily lacerated. Denman confesses that "when women were delivered without assistance, I have not *in any case* observed any considerable laceration."[4] Drs. Hardy and McClintock "frankly admit that when the perineum is not supported laceration occurs much less frequently than might have been anticipated,"[5] and endeavour to explain this fact on the very lame theory that the unassisted woman, through fear, restrains her expulsive efforts at the critical moment of delivery, and has also "been spared the ill effects arising from vaginal examination."[6]

W. Tyler Smith avers that "some of the most practised accoucheurs who have written have stated that they have never known an instance of laceration in secret births, where the woman has been left entirely alone."[7] Other obstetric authors, with evident surprise, acknowledge that nature is usually quite equal to the emergency of the doctor's absence.

A fourth argument in favour of giving up all direct support to the perineum lies in the unfortunate statistics of those who advocate the contrary measure. T. Snow Beck declares that, if the parts were examined by the eye after labour, more lacerations would be discovered than are generally reported. He further states that out of 112 primiparæ whom he attended in labour, 75 experienced lacerations "through the whole extent of the perineum."[8] Simpson consoles himself for his ill-luck with the assertion that in primiparous women the perineum is almost always

[1] British Med. Journ., Feb. 1860, p. 157.
[2] Ibid., p. 196.
[3] Ibid., 1861, p. 487.
[4] Introduction to Midwifery, vol. i. chap. ix. sec. 8.
[5] Leishman's Mechanism of Labour, p. 97.
[6] Practical Observations on Midwifery, Dublin, 1848, p. 9.
[7] London Lancet, March, 1846, p. 321.
[8] Med. Times and Gazette, Feb. 23, 1856, p. 186.

torn, "despite of every modification of management, and in cases in which no kind of management has been adopted;"[1] but, in Great Britain, "no kind of management" does not exclude plenty of lard and plenty of ergot. Figg asserts that "primiparal laceration of the perineum is a ruling event in natural labour."[2] Baudelocque, and almost all the advocates of support, use pretty much the same language; asserting that the *fourchette*—a most convenient word—must yield in first labours; which, in other words, means not only a rent of that mucous fold, but also of more or less of the underlying soft parts. Indeed, after a careful examination of clinical records upon this subject, I have come to the conclusion that the fourchette is the scape-goat of accoucheurs, and that the expression "laceration of the fourchette" is a conventional euphemism for extensive lacerations of the perineum proper. Sedillot is the only author I can find who holds that the fourchette was not made to be ruptured.[3] To those practitioners who boast that they never meet with lacerations in their practice, John Clay retorts that undiscovered lacerations are extremely frequent, and that bloody and distorted as are the maternal parts after labour, "with the finger it is very difficult, and even with the eye it requires a good light, to detect a laceration."

From the foregoing facts it is evident that support directly applied to the perineum has not met with a great measure of success. Let us now examine into the matter more closely and see whether it can stand on its own merits as an adaptation of means to ends. It will be readily granted that the danger from laceration is least when the perineum dilates *slowly* and *equably*. "But mark," observes Leishman, "what happens when we support the perineum. The pressure of the hand being chiefly directed against the posterior and middle portion of the perineum not only subjects a part already on the stretch to an injurious amount of pressure, but prevents the lateral portions of the perineum from bearing their due share of tension. Labour advances, and the time has arrived when the head must complete the curve of Carus and pass under the pubic arch. The whole perineum is now on a stretch, but the lateral portions, having been prevented from undergoing the beautifully graduated dilatation which nature alone effects, does not yield sufficiently. A double strain is now thrown on the central portion, which, especially if the fourchette give way, is thus rendered more liable to rupture."[4] Moreover, the pressure of the hand impairs the vitality of the perineum, by preventing the free circulation of blood; bruised and benumbed, it is no longer a living tissue, capable of responding intelligently, so to speak, to the requirements of the occasion;

[1] Obstetrical Works; Am. ed. 1855, vol. i. p. 330.
[2] Medical Times and Gazette, Oct. 1860, p. 354.
[3] Journal Général de Med., vol. lvi. p. 179.
[4] Glasgow Medical Journ., 1859-60, p. 417.

when to repel, when to solicit the advance of the head, and this nice point nature can determine far better than the hand of the physician. Bonnét[1] teaches that "to support the perineum is one of the most useful duties of the midwife;" and yet, he adds, "gangrene of the perineum has followed faulty compression," an admission which proves the correctness of our proposition, even when a moderate degree of pressure is used.

Again, a majority of the advocates of support contend that it is most needed at the very moment of expulsion, and then by direct pressure upon the posterior commissure. But the woman, in the agony of the final throes, is apt to jerk herself away from the hand of the accoucheur. Of course, then, the perineum, being abruptly released from the counter pressure, is more likely to yield to a strain suddenly sustained, and for which its fibres are not prepared. Ramsbotham recognizes this danger, and, in vivid language, cautions the student against it. Denman admits that "the greatest degree of laceration which ever occurred to me was occasioned by the patient suddenly withdrawing herself out of my reach." Murphy confesses that "one of the worst lacerations that ever came under my notice arose from the patient, an extremely excitable person, suddenly withdrawing herself during a violent pain; the perineum was deprived of its support, and was torn quite through."[2] Smellie, Lee, and other well-known writers, narrate similar cases. Yet, strange as it may seem, it is from such very examples that these gentlemen argue in favour of perineal support.

Further, Madame Lachapelle and Rigby both noticed that pressure upon the perineum increased the expulsive pains of labour, a phenomenon which W. Tyler Smith has explained upon the theory of reflex uterine action.[3] Can it, therefore, be wise to evoke a power which it is desirable to curb? On the other hand, if pressure on the perineum, made from behind forwards, causes the head to hug the pubes and favours its extension, such a measure must necessarily accelerate delivery, and oppose a slow dilatation of the ostium vaginæ. Finally, the word "*support*," as applied to the perineum, is a misnomer. No "support," in the ordinary acceptation of the word, is afforded to the perineum by direct pressure. If such a method ever accomplishes any good, it is by retarding the advance of the head, in other words, by supporting the head through the interposed perineum, and not by supporting the perineum itself. Why not then support the head by pressure directly applied to it, instead of through a medium, which requires perfect freedom from all restraint, in order to undergo the requisite and inevitable amount of dilatation.

Although I believe that the vast majority of natural labours require no assistance whatever to insure a safe delivery, provided the woman has

[1] Cours d'Accouchemens, p. 417.
[2] Brit. Med. Journ., 1861, p. 427.
[3] London Lancet, March, 1846, p. 319.

escaped frequent touching, yet cases do undoubtedly arise which demand intelligent assistance, nor can we always draw the line of demarcation between natural and morbid cases. And let me remark *in limine* that, by laceration of the perineum, I mean any solution of its continuity, be it small or great. For the loss of every fibre entails a corresponding loss of power in the floor of the pelvis, and a consequent impairment of support to the reproductive organs. Where assistance seems demanded to prevent any laceration, the method which I adopt avoids all pressure upon, and direct support to the perineum, and I can the more confidently recommend it, because I make it a rule to have the perineum decently exposed to view, whenever there arises any fear of laceration, and not the slightest fissure can escape my notice. The prudery of obstetric teachers has, I am sure, been the cause of many undiscovered, and consequently uncured lacerations. But, why should a false delicacy interfere with the freedom of manipulation, at a time when a woman's future health and happiness lie at stake, especially since her organs are then no longer *sexual*, in the common acceptation of the word, but *parturient*?

Whenever, therefore, it seems proper to aid nature, insert one or two fingers of the left hand into the rectum, the woman lying on her left side, with her knees well drawn up and separated by a pillow, and hook up and pull forward the sphincter ani towards the pubes. The thumb of the same hand is then to be placed upon the fœtal head, scrupulously avoiding all contact with the fourchette. The right hand need not remain idle, it assists the thumb in making the head hug the pubes, or in retarding its advance; after a pain it presses back the head from the perineum, and thus represses reflex uterine action; it restrains the movements of the woman; it pushes up the corrugated scalp, so that no folds shall remain beneath the sharp edge of the perineum to increase the circumference of the child's head; finally it supports the emerging head and body, causing them to describe the curve of Carus.

I claim for this method the following advantages: (*a*) By pulling up the sphincter ani towards the pubes, not only is nature imitated, which always dilates the anal orifice, but the perineum is brought forward without direct pressure, and its dilatation is diffused over its entire surface, causing a corresponding relaxation of the strain on the posterior commissure in the line of its raphé. In addition, its muscular fibres are crowded up to, and consequently strengthen the line of greatest tension; just as a prudent general hurries up reinforcements to the point of attack. (*b*) The same force which dilates the sphincter ani compels the occiput to hug the pubes and favours extension, especially if the fingers in the rectum are hooked over the prominences of the fœtal face or over the chin. (*c*) This aid is not liable to sudden interruption, for however restless the woman may be, the thumb and fingers, once well applied, follow her movements without relaxing their hold. (*d*) The thumb of the left hand, together

with the fingers of the right, can, by direct pressure upon the presenting part, restrain its too rapid advance, without exciting reflex uterine contraction. (*e*) The circulation of the blood is left free; the nerves are not benumbed by pressure, and the perineum therefore continues in its normal condition, that of a living, elastic, and sentient tissue. (*f*) After the parts attain their maximum dilatation at the occipito-bregmatic circumference of the fœtal head, it is, in my experience, as well as in that of Sacombe,[1] the rapid springing back of the fourchette over the projecting nose, or the subsequent rapid expulsion of the shoulders that often produces lacerations. These causes are, however, well controlled by my method, in the former instance by merely pulling forward the sphincter ani, in the latter, by adding the support of the right hand to the emerging shoulders. Indeed, so effectively do the fingers in the rectum raise the perineum, that the gliding backward of the posterior commissure during the last throe, can, at the will of the physician, be arrested at the bridge of the nose, to be slid off after the cessation of the pain. Finally, as the dilatation of the *ostium vaginæ* is made at the expense of the labia, which are attached to the anterior aspect of the pubic rami and symphysis below the *mons veneris*, much advantage will be gained, both by compelling the complete extension of the head before its delivery, and by carrying forward the perineum in order to *approximate the fourchette to the level of the symphysis*, whence its fibres spring. By comparing a fœtal skull with the female pelvis, it will be seen that its sub-occipital region cannot impinge upon the apex of the pubic arch, but must subtend it as its chord; a vacant space will therefore remain in which the urethra lies secure from pressure. It is only after the head is out of the inferior strait, and completely extended, that its sub-occipital region comes into direct contact with the pubic symphysis, and that on its anterior aspect. If one leg of a compass be placed on the symphysis, and the other upon the bregma of the child's head, at different degrees of extension, it will be seen that the distance from the bregma to the pubes diminishes in proportion as the head is extended, and is least when the sub-occipital region actually touches the anterior aspect of the symphysis, as it does in complete extension. It follows, therefore, that if the head be well extended before its delivery, the greatest dilatation necessary for the *ostium vaginæ* to undergo, will be measured by the occipito-bregmatic circumference *only*, whereas, if the head be delivered before extension is completed—as it is apt to be, unless the fourchette is approximated to the level of the pubes by the two fingers in the rectum—this dilatation must be increased by the addition of the circumference of the space which will exist between the pubic symphysis and the sub-occipital region.

Before adopting this method of managing the perineum, but not since, I

[1] Gardien's Traité d'Accouch., vol. ii. p. 505.

have often found occasion to apply the forceps, in order to extend and deliver the head, when the *ostium vaginæ* was rigid, or the perineum too unyielding for the pains. I do not, however, wish to be understood that the use of the forceps is to be abandoned at this stage of labour; for the forceps will be urgently demanded whenever the vertex bears too much upon the perineal centre, threatening perforation, and I can readily foresee occasions when a force may be required greater than that which can be prudently exerted by the two fingers in the rectum. A perineum is unyielding, or an *ostium vaginæ* is rigid, either because, according to Cazeaux, the perineal muscles are in a state of spasmodic contraction; or because, according to Hewitt, the expulsive pains are disproportioned to the amount of resistance made by the perineal valve. In the latter the forceps may be demanded; in the former very rarely indeed.

During a pain the perineum is tense and inelastic, like the os uteri; in the interval it relaxes and becomes flaccid. Acting on this hint, in cases of rigidity, I am in the habit of seizing the moment of greatest relaxation, that is, immediately after a pain, to enucleate the head by hooking the two fingers in the rectum over the chin, and, at the same time, by gently sliding back the perineum over the parietal bosses. In cases where the pains followed in too rapid a succession to admit of this manœuvre, or the woman was too uncontrollable, or the parts too rigid, I have successfully resorted to full anæsthesia.

There is yet another point which experience has taught me. When the perineum is thin and elastic; when the head advances with a pain and recedes at its subsidence, there is not much danger from laceration. On the contrary, when the perineum is thick, or rendered doughy by a deposit of fat; when it is not elastic, and the head does not recede at the subsidence of a pain, it shall hardly escape laceration if left to nature. In such cases as the latter, whenever the crown of the head begins to appear in sight, and does not recede after a pain, I try to imitate nature by pushing it back, so that the next pain may expend a portion of its force in recovering lost ground. This measure appears to me to be more in accordance with the teachings of nature than is the progressive traction of the forceps.

Delivery by the forceps, even in skilful hands, will often produce laceration; for the head is apt to be brought down too quickly upon the unprepared soft parts, and it becomes a very nice point indeed to determine the exact moment when delivery may be terminated with impunity. The most cautious physician is apt to be caught, as it were, "on the centre." He sees the perineum stretched out to a perilous thinness, and the fourchette almost cracking under the strain. In doubt whether the moment has arrived to raise the forceps-handles and turn out the head, or to depress them and thus restrain its advance, he wavers, and in a twinkling the fibres part. On the other hand, the impatient physician is tempted to turn

out the head before the parts are sufficiently dilated. Finally, what is still more frequent, *hinc mihi lacrymæ*, at the last moment the physician's courage fails him, and he depresses the forceps handles just as the head has begun to emerge; a course equally fatal to the integrity of the perineum. Impressed by these difficulties, so soon as the perineum is well distended, I now take off the forceps, unless their withdrawal requires a force which might accelerate delivery, and either let the woman deliver herself, or else enucleate the head in the manner before described. This practice, if not the more brilliant, is in my hands the safer.

The question now arises whether it is ever justifiable to incise a rigid perineum. In cases of broad and distorting cicatrices resulting from burns, sloughs, abscesses, etc., it may be absolutely necessary to do so, as the only alternative for lessening the bulk of the child's head. But in a perineum rigid *per se*, it seems to me that the necessity for such a course would hardly arise. In the first place, how often, without snapping a single fibre, does the head and breech squeeze through an *ostium vaginæ* so rigidly contracted that no one could presuppose a delivery possible, unless attended with extensive rupture.

In the second place, granting the perineum does not escape injury, my own experience agrees with Kilian's,[1] that such a laceration will heal as readily as an artificial incision, provided a few metallic sutures be at once introduced, and the catheter be used for a few days. The wire suture is decidedly preferable to the serres-fines, which are so much used in France for this purpose, but which approximate merely the cutaneous edges of the wound, leaving the deeper portions gaping. Very little pain is experienced in the introduction of these sutures, provided they are resorted to immediately after the delivery of the placenta; for at this early period after labour the perineum is comparatively insensible.

Before concluding I wish to add my protest to those of Mauriceau,[2] La Bas,[3] Denman,[4] Roederer,[5] Gardien,[6] and Gehler,[7] against the indiscriminate use of lard and of other unctuous substances, so universally found in the lying-in chamber. For this error we are indebted to the ancients, who borrowed the idea from the trainers of their athletes. "*Uncta palæstra*," "*nitida palæstra*," writes Ovid, in allusion to the custom of anointing the bodies of those who were preparing for any contest of strength or of speed. Whilst Lucian, in his dialogue on athletic games between

[1] Die Operat. Geburtshülfe, vol. i. p. 168.
[2] Traité des Maladies des Femmes Grosses, lib. ii. cap. x.
[3] L'Art d'Accoucher, 1780, p. 58.
[4] Practice of Midwifery, vol. i. chap. ix. sec. 8.
[5] Elementa Artis Obstet. p. 146, sec. 311.
[6] Traité des Accouchemens, vol. ii. p. 262.
[7] Schlegel's Sylloge, vol. ii. p. 773.

Solon and Anacharsis, makes the former say: "Since oil renders leather more supple, more tough, and less liable to crack, it would be absurd to suppose that it did not have the same effect upon living flesh." In labours dry from the outset, or that have become so through delay, or through frequent touching, there is advantage in the use of lard, or preferably of glycerine, which induces a serous discharge from the vaginal walls. In my experience, however, the inhalation of ether, and *especially* of chloroform, will act better by restoring the natural glairy secretions. The examining finger, it is true, should always be anointed, so that it may not rob the vagina of its own proper lubricants. But why besmear the maternal passages with oil or lard, and thus dilute and decompose those bland and alkaline secretions which a provident nature has usually so plentifully supplied for aiding the strictly physiological processes of labour? The continental method of keeping the woman on her back has the advantage of allowing these glairy fluids to trickle over and lubricate the perineum. My own practice is to discard the use of lard, to let the vagina alone, and to content myself with keeping the cutaneous surface of the perineum well smeared with the surplus mucus which escapes from the vulva.

The researches of More,[1] Murphy, and others have established the fact that the perineum and vagina, as well as the os uteri, possess, like other sphincter muscles, an inherent power of expansion, apart from the direct pressure of the presenting part of the child; for they often begin to dilate long before it impinges upon them. Is not this expansion in a measure due to the direct influence of the mucous secretions, and also of the liquor amnii? Does it not frequently happen in the outset of labour that a physician experiences difficulty in introducing his well-greased finger into the vagina, a difficulty which no amount of lard will overcome; but, so soon as the secretions are established, or sometimes so soon as the liquor amnii begins to dribble away and moisten those parts, he can readily introduce his whole hand? Cannot the relaxation of a rigid os uteri be sometimes referred more intelligently to an active dilating property in the dribbling waters than to the *vis a tergo* of increased uterine contractions? Whether this be the correct interpretation of their action it is not easy to say; but since nature, from her own laboratory, furnishes the physician with drugs capable of dilating circular muscular fibres, it is not improbable that, to facilitate her own operations, she may provide secretions possessing a dilating as well as a lubricating power?

PRESTON RETREAT, PHILADELPHIA.

[1] Edinburgh Med. Journ., June, 1866, p. 1125.

ART. III.—*Three Cases of Bronchocele successfully removed.* By WM. WARREN GREENE, M. D., Professor of Surgery in the Medical School of Maine, etc. (With three wood-cuts.)

IN December of 1866, I published in the *New York Medical Record* (vol. i. No. 19) the report of a case of large bronchocele successfully removed, and advocated the propriety of an operation in similar cases. I have now the satisfaction to add two other successful cases, and I cannot but believe that a careful examination of them will convince my professional brethren that excision of an enlarged thyroid gland is, under the circumstances which I shall describe, a legitimate operation.

For a brief discussion of this question, I refer to my first report; but in view of the importance of the subject and the propriety of placing the cases in connection, I take the liberty of quoting from that paper the following account of

CASE I.—"Mrs. K., æt. 45, a German lady, residing in Albany, N. Y., consulted me, August 19th, 1866, for an enlargement of the right lobe of the thyroid gland; there was a slight enlargement of the left side. She first noticed the tumour twenty-six years ago, but it had never given her any trouble until within a year and a half, during which time it had rapidly increased to its present size. Its dimensions were now such that the common carotid was crowded behind the posterior edge of the sterno-mastoid muscle, where its pulsations were distinctly felt, and the trachea was pressed firmly to the left side. So great was the pressure upon these organs and the œsophagus, that any attempt to swallow or talk gave her terrible spasms of dyspnœa. She was unable to lie down, and required constant watching during her broken sleep, lest she should die of suffocation. She suffered very much from headache and giddiness, and could not stoop without losing her consciousness. All these symptoms had been for two months rapidly increasing in intensity, and for two weeks almost a daily aggravation had been noticeable. She had recently consulted the leading surgeons of Albany and some others, all of whom told her that nothing could be done. At my request my colleagues, Professors Ford, Palmer, and Storer, and Drs. Smith, Brewster, and others, examined her; and all agreed that a few days at most would terminate her existence, while the thread was liable to snap at any hour. The patient was already aware of her imminent danger, and her only question was as to the *possibility* of relief. I told her in all probability the removal of the growth would be found impracticable; and that even if she survived the operation, which was not likely, the chances were a hundred to one that she would die of secondary affections; but still there was a bare possibility of success, and while I would by no means advise an operation, yet if she, being fully aware of all the facts, insisted upon taking this desperate chance, I would undertake the removal of the tumour. I was happy to have my own views as to the propriety of this advice indorsed by Dr. Storer. She immediately decided upon its being done, and at once. Accordingly, in the presence and with the assistance of Prof. H. R. Storer, Dr. F. K. Paddock, and several students, I made the operation in the following manner: the patient, being etherized, was placed in the ordinary position for ligation of the carotid, and a single straight incision was made over the tumour, extending from the inferior maxilla to the clavicle. The anterior external jugular vein, which ran close by the line of the incision, was not injured. The sterno-mastoid, which was spread over the mass like a thin ribbon, and the several fasciæ, were successively divided upon a grooved director, and the areolar tissue with the knife and fingers, the handle of the scalpel being employed much more

freely than the blade. On raising upon the director the little thin layer of fascia immediately investing the tumour, several veins were wounded and bled profusely; this was controlled by the fingers of an assistant, and the delicate envelope carefully reflected from the gland; but although this was done with the utmost caution and gentleness, several other veins were ruptured. I now found that the entire growth was completely covered with a network of these vessels; and so thin and tender were their walls that the forceps tore, and the ligature cut their coats; and now, although the blade of the knife had not touched the surface of the tumour, so many of these veins were opened, that in spite of all the pressure that could be made the hemorrhage was fearful. I now rapidly separated the areolar attachments, and in a few seconds was at the pedicle, which I found containing three large arteries whose pulsations were very distinct, and which were my guides for dividing the pedicle into three parts, which I also accomplished with the fingers. I immediately tied each third with a ligature composed of eighteen strands of saddler's silk, *saturated* with wax and *loosely twisted*. As I drew the last cord all hemorrhage instantly ceased. The pedicle was carefully divided close to the goitre, and it removed. During the dissection I found at one point the tumour quite firmly adherent to the sheath of the vessels; and while separating it, a gush of venous blood indicated the rupture of a large vessel. The finger of an assistant controlled it until the ablation of the bronchocele, when examination proved the internal jugular to be wounded. This was tied with a ligature of three strands of silk loosely twisted; no other vessels needed interference.

The entire operation occupied twenty-two minutes.

Fig. 1.

"After carefully sponging the wound and allowing the surface to glaze somewhat, the edges were united by interrupted sutures, water-dressing applied, and she placed in bed. The extremities were cool, and pulse feeble but regular, and less than ninety per minute. She reacted nicely, and passed a more comfortable night than she had for weeks. The after-treatment consisted in perfect quiet, water-dressings when agreeable, anodynes *pro re nata;* the exhibition of muriated tincture of iron, twenty drops every four hours, and as much rice broth, gruel, and milk as she would take at regular intervals. For several days there was considerable irritation of the œsophagus and trachea, but not enough to interfere seriously with deglutition or respiration. This passed away, and she recovered without a bad symptom. The last ligature came away on the twenty-sixth day; in another week the wound was entirely healed, and she is now in perfect health and restored to a happy family.

"Figure 1 gives the appearance of the lady before the operation.

"The weight of the tumour was one pound nine ounces avoirdupois."

This lady has continued in excellent health ever since.

CASE II.—Mrs. H., æt. 40, of this city, consulted me in September of 1869, for a tumour, the size of a small orange, occupying the site of the right lobe of the thyroid gland, which was of seven years' standing. She was referred to me by my friend and colleague, Prof. I. T. Dana, who had carefully examined the growth, and whose opinion coincided with my own, that it was probably goitrous. Yet, although presenting the ordinary characteristics of thyroid enlargements, there was an additional feature, extraordinary for a bronchocele of so small size, which threw doubt upon the diagnosis, and was the special reason which led her to seek relief. For a year previous to our examination she had suffered from a constantly increasing difficulty in deglutition. She denied ever having sustained any injury or local inflammation of the parts; but by a careful process of exclusion, both Dr. Dana and myself became sure that a firm connection of the tumour, whatever it might be, with the œsophagus, explained the

dysphagia; and while this was the only symptom which threw a doubt upon the diagnosis of goitre, we felt sure that it, in connection with her persistent and positive denial of any cause that could have produced adhesion of the gland and gullet, justified the suspicion of some other growth taking origin conjointly from œsophagus and trachea.

The trouble in swallowing increased so rapidly that, in spite of all palliative measures, starvation threatened, and she became exceedingly anxious for an operation, possessing, as she did, a full knowledge of all its risks.

Accordingly, October 25th, 1869, I operated in the presence, and with the assistance of Drs. Gerrish, Hunt, Webster, and Getchell, of this city. The patient being etherized and placed in the same position as in the first case, I made an incision five inches long over the most prominent part of the tumour, and divided in succession skin, platysma, superficial and deep fasciæ. The tumour was thus fairly exposed, and was unmistakably an hypertrophied thyroid. Having divided its delicate fascia propria, I proceeded with the greatest care to separate the fascia and enucleate the tumour. This was done almost entirely with the fingers and the handle of the scalpel. The difficulty of this procedure will be appreciated when I state that the deepest portion of the gland was imbedded in what seemed to be a mass of organized lymph through the medium of which it was firmly attached to the œsophagus. So firm was the adhesion, that I refrained from completing the separation entirely with the fingers or instruments, lest I might lacerate the tumour and produce severe hemorrhage. No vessels of large size were found upon the surface of the growth, but two large arteries were felt pulsating powerfully at its base. Between these I passed a blunt curved needle, armed with a double ligature of eight strands of saddler's silk, as near as possible to the œsophagus, and yet separate the vessels. Each portion of the pedicle was then firmly tied, and the tumour removed without the loss of an ounce of blood, and with the ligation of only three small superficial vessels; one end of each ligature being cut short, and the other allowed to emerge from the lower angle of the wound. This was closed with silver sutures, and a light dry dressing applied.

For four or five days the parts were very sore, the efforts at swallowing were very painful, and there was marked throughout severe symptomatic fever. After this time she steadily improved; the wound healed by first intention, except in the track of the ligatures, and these were removed on the 20th day, at which time the dysphagia was almost entirely overcome. More or less of purulent discharge from the opening left by the ligatures continued until the first of December, when the parts entirely healed, and up to the present date Mrs. H. has been in perfect health, with no return of her old symptoms.

The extraordinary adhesion in this case remains a mystery, but from facts in the domestic history of the patient that have since come to my knowledge, I think it fair to assume the existence at some former time of such inflammation as a severe blow or gripe might induce.

CASE III.—In the autumn of 1868, I received a letter from Dr. S. T. Clark, an eminent physician of Lockport, N. Y., describing the case of Miss M. D., of that city, æt. 35, and inclosing photographs, from one of which Fig. 2 is a copy.

From Dr. Clark's communication, as also from a correspondence with the patient herself at about the same time, I learned the following facts:—

The swelling first appeared at the age of fifteen. It attained a considerable size within a few months after it was first noticed, since which time its progress had been very slow. Almost from the first it had induced more or less of dyspnœa. This had latterly increased to an alarming degree, and in connection there had arisen such suffering from dysphagia, headache, and vertigo, with a constant sense of weight and dragging at the tumour, that she was anxious to undergo an operation at any hazard.

I wrote her, portraying as vividly as I was able, the fearful risks of the operation, and stating that I would never, under any circumstances, undertake its performance unless I was morally sure that it gave the only possible chance of obviating an immediately fatal issue, and that I did not feel that she had reached that point; promising her, however, that if she should in the future, in my own judgment and in that of her other medical advisers, reach that condition, I would make the operation if she desired.

In August, 1869, I visited her at Niagara Falls, and examined the tumour carefully. The external appearance is very well shown in Fig. 2, except the increased size; the photograph having been taken nearly a year previous, and the gland steadily although slowly augmenting in volume. The tumour extended from the chin, which it supported in an elevated position, to the sternum, upon which it rested and overlapped in front. Involving both lobes of the gland it pushed outward toward either shoulder, farther on the left than on the right side, and dipped so far down behind either clavicle that I could not touch the bottom. To the fingers, as to the eye, the surface of the tumour pulsated everywhere. Large veins were seen coursing superficially in all directions, and numerous venous channels furrowed on the surface were felt by the fingers. Steady and firm compression notably diminished the size of the mass, although she could not endure it but for a moment on account of the giddiness produced. Upon the right side, midway of the tumour, where the arterial pulsations were most numerous and powerful, there was felt what was diagnosed as, and what afterwards proved to be, an irregularly shaped calcareous mass. At this point there was a very distinct aneurismal thrill and *bruit*. So marked and peculiar was it that an eminent hospital surgeon of Buffalo had pronounced it an aneurism of the common carotid, and told Dr. Clark that he considered the whole tumour as "principally aneurismal." Finding, however, that pressure above the calcareous portion arrested the thrill, I was confirmed in my suspicion that it was due to the very large branches of the superior thyroid coursing over this hard mass.

Fig. 2.

The sufferings of the poor girl were terrible, and her life a burden. She was herself anxious for an operation, but had to encounter the most violent opposition from a very large circle of relatives and influential friends, and also a surgeon of deserved reputation, a near relative. Dr. Clark was the only one from whom she gained any encouragement beside myself, the degree of which I have already indicated. Under the circumstances we advised her to postpone any operation at present, and if she decided to make the trial at last, to leave the question of time to Dr. Clark's judgment.

On the 15th of January, 1870, I received a note from Dr. C., saying

that Miss D.'s condition had become such as to render death liable at any moment. Constant distress from weight and pressure, cephalalgia, vertigo, and dyspnœa to such a degree that her strength was failing, and so laboured was the respiration that she required watching during her broken sleep lest suffocation should occur. His own opinion, with which from his statement mine accorded, was that if ever extirpation was to be attempted now was the time.

I reached Lockport on the morning of the 20th, and found the patient as Dr. C. had described, the tumour having developed in a marked degree since I saw her in the summer previous. Her *morale* was excellent. There were present at the consultation Drs. Clark, Gould, McCollum, Leonard, Palmer, and Foot, of Lockport; Clark, of Niagara Falls; Aberdeen, of Suspension Bridge; Carleton, of Norwich, Conn.; and Gerrish, of this city, all of whom agreed that without interference the case must end fatally in a very short time, and was liable to at any moment. I then said to these gentlemen, and to her personal friends who were present, what I desire here to repeat, so that my brethren in the profession shall clearly understand my position with regard to this operation.

We had to decide whether an attempt at removal gave her a possible chance for life; for my own rule of action is, when a certainty of death stands opposed to a possible chance of safety by operative procedure, to give the patient the chance, no matter how small it is, provided he or she makes the choice with a full understanding of all the facts, and with no prompting by the surgeon. Admitting the extreme danger of such operation, and that the weight of authority, the world over, was decidedly against it, yet I had demonstrated in 1866, and again in 1869, that extirpation of a bronchocele was practicable (in the first case of large size and extreme vascularity), both patients having made a good recovery. But it was certain that this case was more formidable than either of the others—involving both lobes, being of immense size, and more vascular than any goitre I had ever seen; and I did not believe that removal by the ordinary mode of enucleation of morbid growths, attempting to control hemorrhage step by step, was possible. But if my own view, and that which had guided me in the two previous operations, was correct, viz., that surgeons had failed in this very point, and that instead we should always, after carefully uncovering the tumour, avoid the use of the knife, and with handle and fingers go as rapidly as possible to the base of either lobe, where we should inevitably find the two or three arteries which supplied the entire growth; then, forbidding as this case looked, I could conceive success possible. Yet I considered the chances to be a hundred to one that she would die upon the table, or of secondary accident soon after. She had deliberately elected the operation, with a clear knowledge of all its risks, without any advice whatever from myself or Dr. Clark, and in spite of the most determined opposition from all her friends.

After making these statements I asked each gentleman personally whether in his opinion, under all these circumstances, I was justified in attempting the excision, and I was relieved to receive an affirmative answer from each one.

Preparations were immediately made, and in the afternoon of the same day I made the operation in the presence of all the gentlemen above named, and with the especial assistance of Drs. Gould and Clark, of Lockport; Carleton, of Norwich, Conn., and Gerrish, of Portland; and I am deeply

indebted to each of these gentlemen for the calm, quiet, and prompt manner in which they displayed the qualities of the true surgeon.

The patient, taking her place on the operating table, with head and shoulders elevated and firmly supported, was etherized, when I made an incision in the median line from chin to sternum. Dividing the successive layers of fascia upon the grooved director, I came directly down upon the tumour, and began to reflect from it its thin investment. Immediately, as in the first case, at very many points the thin coats of superficial vessels gave way, and uncontrollable oozing resulted. Still, the hemorrhage was not immediately alarming until the dissection, which was carried on by the fingers, reached the calcareous portion of the tumour on the right side. Here adhesions were encountered of considerable firmness, and as they yielded to the most careful efforts I could make, the large branches, which had given the aneurismal thrill, and whose coats were extremely attenuated, burst, and immediately we had the most fearful hemorrhage, such as one who has not seen it can hardly realize. This took no one by surprise, as I had already forewarned my assistants of its probable occurrence, and in such event of the entire futility of any expedient, except the rapid completion of enucleation and seizure of the vessels at the base of either lobe. This I accomplished in a very few seconds, and was enabled so to seize the pedicle with the fingers as to suppress the bleeding measurably until I could transfix it with the blunt needle, armed with double ligature made of eight strands of saddler's silk. This was carried in the median line, from below upward, close upon the trachea, and either ligature tightened sufficiently, by a single knot, to control the hemorrhage and give time for examination. I had hoped to separate either half of the pedicle into as many parts as there were arteries, as I did in my first case. This I found impossible from the firmness of the tissue, unless I used a cutting instrument, which I did not dare to do. I, therefore, tied either half as tightly as possible, and carefully severed the tumour from its attachments. This being done, seven small arterial twigs, not connected with the growth, were tied, when all hemorrhage ceased. The wound was now kept open, only ether enough being administered to keep her quiet, and time given for glazing of the surface and reaction of the circulation. Within ten minutes the vessels in every part of the exposed surface were throbbing violently, and in fifteen minutes from the ablation of the tumour the inferior thyroid artery of the left side escaped from the ligature and spirted with great violence. In my own opinion, as in that of the attending surgeons, this vessel was equal in size to the common carotid in its normal condition. In a moment this was seized and secured; then, the entire left half of the pedicle being transfixed with a tenaculum, a new ligature of twelve strands was carried underneath both the original one surrounding the pedicle, and the separate one around the inferior thyroid artery, and drawn as tightly as possible. From this time all bleeding was arrested, and in a few minutes the wound was closed with silver sutures, a light dry compress applied, and the patient placed in bed with a fair pulse of 100 per minute, and presenting no marked signs of collapse. She took some gruel in the evening with little difficulty, and passed a more comfortable night than she had for years. The following afternoon at two o'clock I left her entirely comfortable. For the subsequent history of the case I am indebted to Dr. Clark. Everything went well until the tenth day, when secondary hemorrhage occurred, which was controlled by digital compression, which was continued for seventy-two hours. Whether it was neces-

sary or not for so long a time it is impossible to say, but it is an illustration of the caution and prudence which characterized Dr. C.'s excellent management throughout. During this time, and for some days subsequently, all nourishment was administered by enemata, only small lumps of ice being given by the mouth. With this exception the convalescence was uninterrupted. The last ligature separated on the twenty-first day, and within four weeks from the operation she was off the bed. She has since remained in excellent health. One week before the operation she lost her voice entirely. Since the removal of the tumour she has fully recovered it.

I regret exceedingly to have failed in securing a photograph of Miss D. since her recovery. In answer to a letter requesting one, Dr. Clark writes, under date of October 20th, 1870: "Miss D. has had several sittings since her recovery, but has failed to secure a picture to suit her, perhaps too fastidious, taste. I doubt if the scar would show at all in a photograph, and is growing beautifully less. She is in excellent health."

Fig. 3 represents the appearance of the gland after section. Unfortunately for the purpose of illustration, as also for an accurate estimate of its weight, the tumour was cut open and soaked in water for several days before its photograph or weight were taken. By this means not only were the numerous large bloodvessels emptied of their contents but large masses of soft atheromatous matter scattered throughout the fibrous stroma were dissolved. The weight upon removal, as nearly as could be estimated, was five pounds.

Fig. 3.

In closing the report of this case I must again bear witness to the most excellent assistance I received during the operation, and to the unremitting and skilful care which Dr. Clark and his *confrères* bestowed upon the case afterwards. With such assistance a surgeon can certainly do his best; and with such after-treatment a patient has all the advantages that professional skill offers.

I prefer to submit these cases to the profession with very little comment. They are the only ones in which I have ever attempted the excision of bronchocele, and if they are the last I shall not regret it. For while their issue has been so fortunate, I am sure that no man could witness even, much less perform, these operations, and envy the man upon whose lot it fell to undertake them. Yet under similar circumstances I should not shrink from such responsibility, and this for the reason that the possibility of successful extirpation, even of the worst cases, is established; and I believe the operation, performed in the manner I have indicated, may claim quite as secure a place among legitimate *dernier resorts* as amputation at the hip-joint.

The several steps of the operation, as I perform it, are briefly these—

1st. Exposure of the tumour by linear incision of ample length, avoiding most sedulously any wounding of the tumour or its fascia propria.

2d. Division of the fascia propria upon the director.

3d. Its reflection and the enucleation of the tumour with *the fingers and handle of the scalpel*, paying no attention to hemorrhage, however pro-

fuse, but going as rapidly as possible to the base of the gland and compressing the thyroid arteries.

4th. Transfixion of the pedicle from below upward with a *blunt* curved needle armed with a double ligature, and tying each half, or, when practicable, dividing the pedicle into as many portions as there are main arterial trunks and tying each portion separately.

5th. Excision of the gland and subsequent dressing of the wound as in ordinary cases.

In conclusion, I cannot refrain from one word of warning to my younger brethren whose ambition may make their fingers tingle, lest they should in the light of these successful cases be too easily tempted to interfere with these growths. It is and always will be exceedingly rare that any such interference is warrantable; *never*, for relief of deformity or discomfort merely; *only*, to save life. And if it is beyond all question, determined in any given case, that such an operation gives the only chance for snatching a fellow-being from an untimely grave, be it remembered that accurate anatomical knowledge, and a perfect self-control under the most trying ordeals through which a surgeon can pass, are indispensable to its best performance.

PORTLAND, November 2, 1870.

ART. IV.—*Morbid Growths connected with the Nervous System: Cerebrum, Cerebellum, and Semilunar Ganglion of the Sympathetic.* By ROBERT T. EDES, M.D., Assistant Professor of Materia Medica in the Medical Department of Harvard University, Boston. (With two woodcuts.)

CASE I. *Morbid growth in cerebrum.*—Miss T., æt. 83, noticed in February, 1869, that she had considerable difficulty in speaking, and this increased. In September marked paresis of right side of face was noticed, and, some time after, a similar condition of right side of body. In September she said only a few words with great difficulty. For about a month before death she did not speak at all, and swallowed with great difficulty. Had frequent attacks of coma. Finally both sides were paralyzed, and feces were passed in bed. Died November 6, 1869.

The patient was a lady of great intelligence, but minute inquiries into her condition were rendered difficult by her extreme deafness. There was no reason to suspect aphasia nor any disturbance of the intellectual functions, certainly for long after the first symptoms manifested themselves.

Autopsy twelve hours after death, by Dr. D. Coggin, her attending physician.—Dura mater congested. Anterior and parietal lobes of brain shrunken, convolutions separated by wide sulci. Considerable serum under arachnoid. Posteriorly, convolutions compressed.

On left side two small convolutions of parietal lobe had a peculiar rosy hue. This corresponded to small yellow masses, apparently distinctly limited, beneath cortical substance. Near one of them was a small hemorrhagic mass about one-eighth by one-half inch. One or two more small similar masses were found on slicing down brain.

In parietal and occipital lobes was a large greenish-yellow mass, above and behind the posterior cornu of the ventricle. In this mass was involved gray matter belonging to the sulci at the base of the brain and to the sulcus of the hippocampus minor. It was composed in some parts of flesh-coloured or grayish material mixed with the more abundant greenish puriform matter, and had very much the appearance of a large abscess formed by the coalescence of several small ones. It had no distinct capsule, and its boundaries from the gray cerebral substance could not in all parts be distinguished. It was about an inch and a half in diameter, nearly round, and occupied a large part of the posterior lobe. A similar mass, the size of a large walnut, was found below and anterior to posterior cornu. Neither of them appeared upon the surface of the brain. Cerebellum and pons apparently healthy. Thorax and abdomen not examined.

Microscopic Examination.—Capillaries nearly everywhere filled with blood. In some places, especially at the bottom of a sulcus near the sulcus centralis, round bodies were found in the outer layer, which took a reddish-brown colour with iodine (amyloid bodies).

Sections of the medulla oblongata show in the anterior part of the pneumogastric nucleus, just external to the hypoglossal nucleus, several large, very strongly pigmented cells, contrasting markedly with the healthy cells in their immediate neighbourhood. The large masses of disease (examined in the fresh condition) appear to consist of masses of cells not very different from those of pus, but more irregular in their form. No hæmatoidin in crystals or granules. Many large cells with fat-globules ("exudation-corpuscles").

Satisfactory sections could not be obtained from the larger masses, but from the smaller, which were apparently of similar structure, several were obtained which showed (and still do so) quite well the general formation and in some parts the single elements. At the outer surface of some portions an external layer, undoubtedly corresponding to the normal external layer of the cerebral cortex, is well marked. It consists of finely-granular intercellular substance, with fibres of a character hereafter to be described, and cells resembling those found normally in the same situation, but more numerous. Next below this layer, which is of about the usual thickness, the cells take a direction more or less parallel to the surface—that is, at right angles to the direction of the normal pyramidal elements. This course seems, from the connection of bloodvessels entering from the pia mater, to be determined by the presence of other bloodvessels taking this direction. Below this the cells are more closely and irregularly packed, so that it is difficult to make out the precise arrangement; but as the section approaches a softer part of the growth, its edges become torn and ragged, and allow the individual elements to be made out with considerable distinctness. Most of these are elongated or spindle-shaped, or more or less triangular, in a portion the nucleus being distinguishable. Where the nucleus cannot be seen, it is probably often concealed by the hardening of the preparation or the very deep staining by carmine. Some of these are represented in Fig. 1, *a*. In other places very much elongated spindle cells (*d*) were found, a few normal cells somewhat larger than the average of the others,

and a *very* few (one or two) which resembled the larger pyramidal cerebral cells. The majority of the cells had one or more processes, which gave to those of triangular or pyramidal form (*b*) a strong similarity to the smaller pyramids normally present. Beside these processes, and without evident connection with them, were many fine fibres intermingled with the cells. Similar fibres were seen in other portions of the brain, and were very possibly, partly at least, the result of the method of preparation. It is not certain, however, that all or a large part of them may not be prolongations of the processes arising from the cells. The arrangement was irregular, not at all resembling that of the finer normal nerve-fibres.

Remarks.—The microscope showed in this case that the abnormal portions of the brain were not what they much resembled, to wit, abscesses or the remains of apoplectic effusions.

If we follow the classification of Virchow, there is but little difficulty in attaching them to his "glioma," as, indeed, was to be suspected from the locality.

Had the examination in the fresh condition been more thorough, it would probably have shown that the greenish colour and semifluid consistence of large portions of the two principal masses were due to fatty degeneration, and it is possible that some of the cellular elements might have been more distinctly marked out by this process than in the other and firmer portions. The less degenerated parts showed the naked eye characteristics of "glioma" in their great resemblance to the normal brain-substance, their rosy colour in some places, and the indistinctness of their separation from the healthy substance. It will be seen by reference to the description which applies especially to one of the pinkish convolutions from the parietal lobe, that the hyperplasia had actually reached the surface, and that the convolution in question was actually a part of the glioma, and not merely in its immediate neighbourhood.

The basis of the diagnosis may be seen by comparing the description and drawings of the microscopic characters, both of which were prepared before I had read Virchow's description, with the following passages from the "Morbid Tumours"[1]:—

"If the neuroglia is hardened in alcohol or chromic acid, in thin sections, a kind of very fine network may be seen. The substance appears as if it were penetrated by delicate fibrils crossing each other in every direction, of which it is hard to say whether they pre-exist or are formed from the original substance by coagulation."

"Gliomas of the brain-substance, which furnish the purest form of the hyperplasia, allow, in the recent state, complete cells to be only with difficulty recognized; they appear, like the normal neuroglia, to consist only of a granular substance with nuclei. After hardening, the network of fibrils already mentioned may be seen."

"Sometimes there are very small cells placed closely around the nuclei, which possess very fine, simple processes, and anastomose with each other like the so-called granules of the cortical substance. At other times the cells are larger—that is, the cell-bodies more voluminous—the cells themselves either round [see Fig. 1, *c*], spindle [Fig. 1, *a*] or star-shaped, in the latter case with long pro-

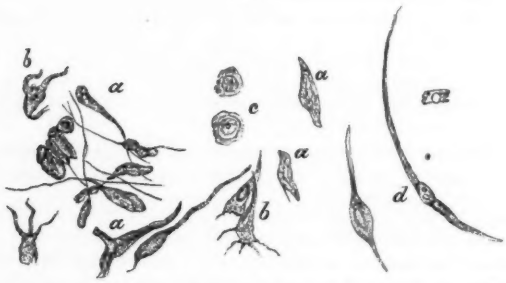
[1] Die krankhaften Geschwülste, II. Band, I. Hälfte, p. 128 et seq.

cesses [Fig. 1, *b*]. Among these cells it is often possible to isolate single very long structures which look like simple fibres, but in certain parts contain a nucleus in a spindle-shaped dilatation [Fig. 1, *d*]. They are sometimes like the radiating fibres of the retina, sometimes like the long spindle cells which I find between the elements of the pineal gland and hypophysis cerebri. Most frequently they lie in the immediate neighbourhood of vessels around which they sometimes form a thick sheath; yet they are also found away from the vessels, in the midst of the other tissues."

"The nervous elements which normally are situated in the neuroglia, and are surrounded by it, are not found in the tumour."

In this case the two cells marked *b*, Fig. 1, certainly very strongly resemble the smaller pyramidal cells of the cerebral cortex, and I also found

Fig. 1.



Elements from Section of Diseased Convolution of Parietal Lobe. *a*. Fusiform cells and fibrils. *b*. More or less triangular cell processes. *c*. Round cells. *d*. Long fibre-cells.

one or two larger ones which much resembled them. The distinction, however, between the connective tissue and the proper nervous elements of the brain is not yet decisively made out, and it would be very difficult to distinguish in all cases some of the smaller cells of the outer layers from irregular forms to be found in a morbid growth.

CASE II. *Morbid growth in the cerebellum.*—Miss ——, æt. 60. About three years ago a tumour, considered scirrhous, was removed from left breast. Wound healed well but slowly. Several weeks before death complained of dizziness and nausea which yielded to treatment. Nausea returned at times, probably from irregularities of diet. Died Jan. 9, 1870.

Autopsy Jan. 10.—Body emaciated. Cicatrix on left breast thin, not adherent; no tumour in that region. A lymphatic gland dissected from beneath pectoralis major somewhat enlarged.

Heart soft; left lung adherent; liver, kidneys, stomach normal; spleen small.

Cyst on each side of uterus, apparently degenerated ovaries; on left side as large as horse chestnut, filled with glutinous material of light gray colour; on right side as large as small cherry, filled with tarry black material.

Head. Considerable escape of serum during removal of calvaria, which was accomplished with some difficulty, owing to adhesions. Cerebrum and base of brain apparently normal.

On cutting into cerebellum the incision on the left side passed through a nearly spherical tumour about an inch in diameter, situated posteriorly and externally to the valve of Vieussens, above the middle peduncle of the cerebellum, and resting on these structures at the posterior and external portion of the pons and crura cerebri. The corpus dentatum cerebelli was pushed downward and backward. The tumour was distinctly limited in whole extent. Superiorly and posteriorly a few laminæ of the cerebellum passed outward over the surface of the tumour, and were more closely united with it than any other portion.

Colour externally not very different from that of surface of cerebellum, passing into a dingy yellowish-white internally. Beneath the cerebellar folds last mentioned there was a shallow fissure, as it were an indication of a division into lobules. Texture soft and friable. Remainder of cerebellum on both sides apparently healthy. Superior surface of left crus cerebri and the pons Varolii where pressed upon gave no indications of disease to the naked eye.

Microscopically the tumour was found to consist of large cells, roundish, or more or less irregular in shape; none, however, fusiform or caudate. The nuclei rather large, some with nucleoli. Several cells held two nuclei each.

As the centre of the tumour was approached the cells showed more fatty degeneration, and in portions but little was to be seen beyond a mass of fat granules with abundant nuclei intermixed. It was noticed that on crushing or picking apart prepared pieces of the tumour that cylindrical masses of these cells inclosed each a bloodvessel, and that these masses took a deeper colour from the carmine than portions more distant from the vessels. There was no stroma whatever of the character found in cancer, and very little intercellular substance of any kind.

The lymphatic gland from beneath the pectoralis major showed in some portions a structure similar to that described, though less clearly to be made out.

Remarks.—This tumour may perhaps be most appropriately designated a round celled sarcoma, which is a form frequently found in connection with the nervous centres, though the diagnosis is not altogether clear. Virchow[1] says :—

"In fact there is no more difficult problem in oncology than under certain circumstances to separate the round-celled medullary sarcoma from carcinoma. The diagnostic criterion is the relation to the intercellular substance. So long as one perceives in the cells a tendency to the separation of intercellular substance, or a preservation of their relation to the intercellular substance, so long can one be certain that he has before him no cancer, for this latter is marked by its cells keeping themselves distinct and entering into nearer relation only with other cells. Stress is therefore correctly laid upon the alveolar structure of cancer, and the filling of its alveoli with closely pressed epithelioid cells."

The diseased lymphatic gland renders it very probable that this case was no exception to the rule that the "round celled sarcoma is the most malignant of all non-carcinomatous tumours,"[2] and it would have been very interesting to know whether the original tumour removed from the

[1] Op. cit., II Band, p. 201.

[2] Rindfliesch, Lehrbuch der Pathologischen Gewebelehre, p. 121.

breast was a true carcinoma or a malignant sarcoma, the former supposition being the more probable from its seat, and the latter from its supposed consequences.

CASE III. *Morbid growth of the semilunar ganglia of the sympathetic.* —Mrs. B., æt. 71. Three years ago pain and difficulty in urinating. Secretion scanty. Two years ago had pain in back over left kidney, extending through to bowels, and accompanied by a burning sensation.

Jan. 1869. Pain in back shooting in various directions. Tenderness on pressure.

March 15. It was thought that left kidney could be distinguished on pressure. General health improved a little for a few weeks, when pains returned. Tongue became red and somewhat tender. Extremities inclining to coldness. Pulse always remarkable for regularity, about 65, soft and of good strength. Morning before death, when wholly insensible, pulse steady and regular though weak. For last two or three months has taken nutrient enemata on account of nausea and loathing for food. No vomiting. Constipation has been very uniformly attendant upon the case. Died Nov. 1869.

A specimen from this patient given to me by Dr. Blanchard, the regular medical attendant, and Dr. Cushing, who had seen her in consultation, consisted of a portion of the abdominal aorta with the pancreas and left kidney attached. The kidney, about the identity of which some doubt had existed at the autopsy, was clearly shown to be such by the passage into it of a probe from the ureter, by its position in regard to the renal artery of the other side and by some remains of its normal structure which were in one or two places distinguishable by the naked eye, and still more so by the microscope.

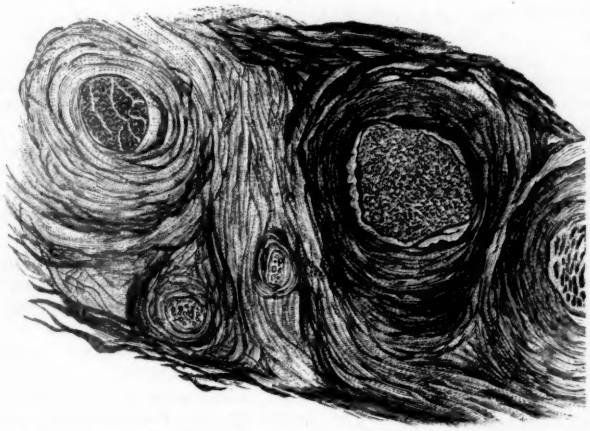
The diseased kidney was of about the normal size, closely adherent by fibrous tissue to the left side of the aorta, and could not without violence be distinctly separated from it or its branches. A section made in the usual direction showed several thick layers of whitish brawny substance upon the outside, between which, and also in the region where the pelvis, ought to have been, were irregular cavities filled with darker soft material, some of which existed also in the pancreas. A small portion, probably of the supra-renal capsule, adhered to the kidney, and presented the appearance of a coarse fibrous network filled with a reddish jelly.

On slitting open the aorta, it was seen that the left renal artery was contracted so that its calibre would hardly admit a large pin. There were a few spots of atheroma, one at the entrance of the renal artery.

Behind the kidney were two ganglia of the sympathetic nerve, and above, and connected with these, were two large nerves, probably the splanchnic, which passed behind the kidney into the mass of fibrous material between it and the aorta. The upper and anterior one sent a branch to one end of a hard fibrous mass on the left side of the cœliac axis, which corresponded in situation to the semilunar ganglion of the other side. These nerves soon became thickened and involved in the fibrous mass before mentioned, and could be traced distinctly but a short distance therein. The cœliac axis and superior mesenteric artery were surrounded by the fibrous tissue, with which the nerves became apparently continuous, and which extended downward three-fourths of an inch or an inch, and had no very definite lower border. The branches of the epigastric plexus also entered this mass.

It is difficult to decide, in the midst of the various alterations which have taken place, what is the essential nature of the disease, but by exa-

Fig. 2.



Section of semilunar ganglion, showing bundles of nerve fibres surrounded by connective tissue.

mining those portions where there is little or no normal tissue to be distinguished, we find a structure which it is fair to consider as characteristic of a portion, at least, of the mass, and which furnished a key to its classification.

In some places, on the edge of thin sections, we find the structure to consist principally of fibrous bundles with elongated nuclei, not unlike the appearance observed in fibrous tumour of the uterus.

At other places, where the section is more torn, and in the fresh condition was probably softer, cells of an elongated and sometimes fusiform shape are found with nuclei like those above mentioned. There are, especially in the torn portions, indications of a stroma, but where the section is thin, and at the same time entire, a proper alveolar structure is not to be made out, although the remains of kidney tubes inclosed in hypertrophied connective tissue present somewhat this appearance. The stroma is probably the intercellular substance of a sarcoma, or the remains of normal tissue.

It is fair to say that, in some portions, the cells become so large or so irregular and crowded, that one is almost inclined to consider the growth a true carcinoma, rather than anything less malignant. But, considering the more prevalent structure, the absence of proper alveoli and stroma, and bearing in mind that we are liable to a certain admixture of normal elements, as is occasionally quite evident, it seems more correct to consider the growth a sarcoma varying in character from the cellular structure last described to an almost purely fibrous character which we shall find in other portions.

The absence of infection of neighbouring parts, except by close contiguity and contact, also favours this view.

The alterations in the glandular structure of the kidneys, when this is partially preserved, are of two kinds.

In one, there is hypertrophy of the intertubular fibrous tissue, with many nuclei, the increase being much more general and uniform than in the usual cirrhotic or contracted kidney.

In the other, the whole structure is degenerated into a nearly homogeneous structureless mass, in which, however, here and there may be distinguished the basement membrane of tubes, and an occasional longer or shorter row of cells, distinguished by their much deeper tint from the carmine, and representing the epithelial lining of tubes. Where a tube has been cut across, or where a good longitudinal view is obtained, it appears as if an entirely structureless exudation occupied the interior of the basement membrane, and, where the epithelium is preserved, had crowded this into a narrow space in the middle. In some places there is a considerable amount of the dark bordered, curled, connective tissue fibres.

In the nervous ganglia and their neighbourhood, the hypertrophy of fibrous tissue is great, and constitutes the fibrous mass above described as involving the cœliac and mesenteric arteries, their accompanying plexuses, and the semilunar ganglion of the left side. The bundles of nerve fibres are surrounded by thick concentric annular masses of connective tissue, in most, though not all instances, non-nucleated.

The amount of connective tissue within the ganglia was much greater than normal, as I have convinced myself, by examination of the same part in other elderly persons, although a certain amount is always present. There was no evidence, however, of any degeneration of either tubes or cells in consequence of pressure. A small amount of cellular atrophy, however, could only be determined by measuring many cells in comparison with normal specimens. This was not done, but the appearance of the cells indicated no material lesion.

Remarks.—The chief point of interest in the anatomy of this case is the condition of the semilunar ganglion and the nervous plexuses connected therewith. Reported cases of lesion of sympathetic ganglia are very rare. Recently several cases have been reported where fibrous hypertrophy, with atrophy of the true nervous elements, has been detected in the cervical ganglia, but I have found only one instance which seems to have been similar to the present, and it is this fact which has induced me to publish a case otherwise without features of great interest.

M. Charles Robin observed a tumour of the solar plexus, formed of nervous cords ramified and anastomozed. The cords were enlarged, grayish, semitransparent, a little gelatiniform at centre, composed largely of connective tissue and amorphous material. In the masses corresponding to ganglia were ganglionic cells similar to the normal.[1]

It is hardly possible to connect any of the symptoms observed with the affection of the ganglia, since the pain had nothing characteristic about it, and constipation is too common a symptom in elderly women to be traced to any such rare lesion; beside which, it may well be doubted whether any injurious compression had as yet been exercised upon the nerve tissue proper.

[1] Cited by Polaillon, Journal de l'Anatomie et de la Physiologie, 1866.

ART. V.—*Pigment Induration of the Lungs.* By FRANCIS DELAFIELD, M. D., Curator of Bellevue Hospital, New York.

THERE is a lesion of the lungs caused by heart disease, which has attracted but little attention, and yet is both of frequent occurrence and of considerable importance.

The first detailed description of this lesion was given by Virchow,[1] in an article on pathological pigments, published in 1847.

"There is a process," he observes, "which, in spite of its frequency and characteristic appearance, has attracted but little attention. It occurs in patients who have suffered for a long time from asthma or chronic bronchitis, dependent upon disease of the left heart and accompanied by hæmoptysis. Insufficiency and still more frequently stenosis of the mitral valve most frequently produce it. When the thorax is opened, the lungs appear prominent, do not collapse, are compact, heavy, inelastic, crepitate but little, and have frequently a peculiar yellow, brown, or reddish-brown colour. On section the tissue is denser and at many points are red spots, dark coloured at their centres, lighter at their edges, also other spots of a brown, rusty, or black colour. The parenchyma between these spots is seldom of its normal colour, but is yellow, orange, or rust-coloured. A yellow or brown, slightly frothy fluid, escapes on section in many cases very abundantly. Microscopical examination of such lungs shows only the normal tissue, with extravasated blood, in different stages of transformation into pigment. In the neighbourhood of the points of extravasation the epithelial cells of the alveoli are stained yellow, and it is easy to see that these cells are identical and continuous with the colourless pavement epithelium. There are also other cells stained in the same way, but containing in addition granules of pigment. These granules show all gradations of colour from yellow to black. Finally the cells may be destroyed and the pigment left free. In addition extravasations of blood take place in the interstices of the tissue, and these become changed into pigment.

"This lesion of the lungs is produced by a chronic hyperæmia. There is at the same time chronic bronchitis with hyperæmia and thickening of the mucous membrane of the bronchi and dilatation of their calibre. Any thickening of the walls of the air-cells is hypothetical. I consider the most appropriate name for this condition of the lungs to be brown or pigment induration."

Some time before this, Andral[2] had described a form of induration of the lungs as produced by a hypertrophy of the walls of the air-cells, with diminution of their cavities and accompanied by a red, yellow, gray, brown, or black pigmentation. These different colours he considers to be merely modifications of the same substance.

Hasse[3] had also described still more particularly a form of chronic pneumonia occurring in connection with hypertrophy of the heart.

"The diseased lobes were but moderately distended; their colour was of a dark, brownish-red, both their absolute and specific gravity augmented, and their consistency greater than usual. Their parenchyma appeared consolidated, nowhere permeable, nowhere crepitant, nor yet saturated with blood or serum. The cut surface was smooth and even."

Förster[4] states that "in chronic hyperæmia of the lung, produced by heart

[1] Virchow's Archiv, i. 460. [2] Andral, Anat. Path., ii. 517, 1832.

[3] Hasse, Path. Anat., 1846, Am. ed., p. 219.

[4] Förster Path. Anat 1863, ii. 240.

disease, there may be, a formation of pigment in the connective tissue and epithelium, accompanied by a dilatation of the capillaries and by congestion."

Rindfleisch[1] says: "The lungs appear heavy, but not œdematous, contain a moderate amount of air and have a brownish-yellow colour. The microscope shows a moderate increase of the connective tissue of the walls of the air-cells, with an increase in the length and diameter of the capillary vessels."

Pathologists, therefore, have recognized the fact that chronic hyperæmia, usually produced by heart disease, may give rise to structural changes in the lungs, and that these changes consist in increase of pigment, hypertrophy of the walls of the alveoli, and dilatation of the capillaries.

If we turn to the clinical writers on chest diseases, we find but little information. Stokes[2] remarks that mitral stenosis may give rise to symptoms of congestion of the lungs, namely: cardiac asthma, bronchitis, hæmoptysis, œdema, and pulmonary apoplexy; and, Walshe[3] says that as the results of mitral disease, there may be cough, watery expectoration, dyspnœa and orthopnœa, bronchitis, pulmonary œdema, pneumonia, passive congestion, and pulmonary apoplexy.

Skoda[4] mentions this lesion under the name of "Hypertrophy of the Lung."

"The lung tissue is firmer and denser, without any increase of blood or serum. The change consists in an increase of the connective tissue of the walls of the air-cells, with narrowing of the cavity of the air-cells and often with an increase of pigment. The percussion tone is unaltered, the breathing is vesicular unless bronchitis is present."

Latham[5] merely observes, "that the lungs are the most frequent seat of the congestions and effusions, hemorrhages and inflammations, which are the results of an unsound heart;" and Fuller[6] "that obstruction of the mitral orifice causes congestion of the pulmonary system and proves fatal with congestion, œdema of the lung, and pulmonary apoplexy."

Oppolzer[7] says: "That with mitral stenosis and insufficiency the veins and capillaries of the lungs become dilated, and the pulmonary artery and its branches may also be dilated."

Neither Flint (*Treatises on the Heart and Lungs*), Waters,[8] Wood, Da Costa, nor Loomis mentions this lesion.

In Aitken's *Practice of Medicine* it is merely stated that mitral disease of the heart is accompanied by congestion and œdema of the lungs.

Niemeyer[9] observes, "A second form of hypertrophy of the lung, described by Virchow as pigment induration, consists in an increase of the tissue of the lungs at the expense of the alveoli. It is observed in chronic hyperæmia of the

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[1] Rindfleisch, Path. Geweb., p. 370.

[2] Stokes, Diseases of the Heart, French ed., p. 195.

[3] Walshe, Diseases of the Heart, Am. ed., p. 289. In his treatise on the Lungs, there is no further information on this subject.

[4] Skoda, Percussion und Auscultation, 6th ed., p. 282.

[5] Latham, Diseases of the Heart, 1847, p. 320.

[6] Fuller, Diseases of the Chest, 1862, p. 562.

[7] Oppolzer, Krank. des Herzens, 1867, p. 199.

[8] Waters, Diseases of the Chest, 1868.

[9] Niemeyer, Lehrb der Path. and Ther., i 117.

lungs, produced by disease of the mitral valve and hypertrophy of the right ventricle. The walls of the alveoli are thickened, the alveoli smaller, the lung not increased in size, its tissue denser. The colour is brownish with black spots. This pigmentation depends upon capillary hemorrhages in the tissue.

"This lesion may be suspected during life, but can never be certainly diagnosticated."

The works, therefore, which serve as text-books for most students, either do not mention this affection at all, or advert to it as a rare disease not to be diagnosticated.

My observations are based on a study of twenty autopsies of this disease.

The lesion is not an uncommon one. During a period of twenty months there were made, at Bellevue Hospital, sixty autopsies in which there were marked lesions of the heart. In twenty of these cases, one-third of the number, pigment induration of the lungs was found.

The appearance of these lungs is very characteristic. When the thorax is opened, the lungs appear small, usually several old adhesions connect the pulmonary and costal pleura, and bind together the different lobes. In the pleural cavities is usually a moderate amount of clear serum. When the lungs are removed, their small size is still more apparent. The pulmonary pleura is thick, opaque, and gives the surface of the lung a grayish colour, mottled by the pigment in the lung substance beneath it. When the lungs are cut open, the colour and texture are very characteristic. The texture is dense and resistant, like that of compressed lung. It is very dry, contains very little air, hardly any blood or serum escapes. In some cases, however, the lung tissue is œdematous but not congested. Sometimes irregular portions of lung tissue are still firmer, and break down under the finger, but do not present the granular appearance of croupous hepatization. Not infrequently hemorrhagic infarctions are found. The colour is a very marked, uniform, bright pink. This pink may be mottled with spots of brown or black in a very variable degree. The increase of pigment on which Virchow insists, although present in nearly all the cases, was very seldom of sufficient amount to attract attention with the naked eye.

The red hepatization did not resemble that of croupous pneumonia. It occurred in irregular patches, sometimes to a small extent, sometimes involving nearly an entire lobe. The small patches might easily be overlooked.

In all the cases the same changes were found in all the lobes of both lungs.

The microscopical appearances were also very characteristic. Four pathological conditions in variable relative proportions were always found: (1) new pigment; (2) hypertrophy of the walls of the air cells; (3) dilatation of the capillaries; (4) a marked increase of cellular elements within the alveoli.

The new pigment corresponded pretty closely to Virchow's description,

except that in most cases it existed in a less degree. No extravsations of blood, however, were observed.

Hypertrophy of the walls of the air-cells was found to exist in very different degrees. In some cases it was very excessive, almost like the fibrous induration of phthisis; in others it was hardly appreciable.

Dilatation of the capillaries existed, to some degree, in all the cases. Not only was their diameter increased, but they were also longer, so as to assume a tortuous shape, and project in loops into the cavities of the alveoli. They usually contained but few blood globules.

An increase of cellular elements in the walls of the alveoli was always present. In the adult human lung, a regular epithelium lining of the alveoli cannot be demonstrated. There may be seen, however, shreds of a transparent, nucleated membrane and a very few isolated, large cells, of round or polygonal shape, with a single nucleus. These cells are found free in the alveoli or loosely adherent to their walls. They are found in increased number and in various stages of fatty degeneration, but mixed with lymphoid cells and fibrin, in croupous pneumonia.

These same cells are found in pigment induration. No alveoli can be found, which do not contain an increased number, while many alveoli will be completely filled. When the alveoli are thus filled over a considerable area, there are patches of red hepatization visible to the naked eye. But even when no hepatization can be detected by the eye or touch, minute examination shows always a very considerable increase of these same elements. Fibrin, granular matter, and lymphoid cells are absent. This partial or complete filling of the alveoli with new cells is the most constant lesion in pigment induration, and certainly not the least in pathological importance, yet it is the one which has attracted the least attention.

The condition of the heart, which occurs most frequently with pigment induration, may be seen from the following table taken from sixty cases of marked cardiac disease:—

Mitral stenosis with hypertrophy of the right ventricle:—
Pigment induration present 8 *times, absent* 1.

Mitral insufficiency, aortic obstruction, or insufficiency with hypertrophy:—
Pigment induration present 6 *times, absent* 16.

Mitral insufficiency with hypertrophy of the left ventricle:—
Pigment induration present 4 *times, absent* 6.

Old pericardial adhesions, without hypertrophy or valvular lesions:—
Pigment induration present once.

Mitral valves covered with vegetations, no hypertrophy:—
Pigment induration present once.

From these cases it appears that mitral stenosis is almost always accompanied by pigment induration of the lungs; that simple mitral insufficiency

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causes it in nearly half the cases; and that aortic disease with mitral insufficiency causes it in one-fourth of the cases.

In the cases of mitral stenosis, the leaves of the valve and chordæ tendineæ were thick and rigid, the orifice of the valve was converted into a narrow slit, and the entire valve assumed a funnel-shape with the point towards the ventricle. There was always some hypertrophy and dilatation affecting both the right and left ventricles. In two cases the aortic valve was atheromatous; in one case rigid and stenosed; and in one case the tricuspid valve was thick and more than usually insufficient.

In the one case of mitral stenosis without lesion of the lungs, the ventricles were hypertrophied, the mitral valve was stenosed, calcified, and covered with vegetations; the lungs were normal.

In the cases of mitral insufficiency with aortic valvular lesions: in three cases the aortic valves were covered with large vegetations, and both ventricles were dilated and hypertrophied; in one case the aortic valves were calcified and stenosed, the mitral valve was insufficient and calcified, and both ventricles were dilated and hypertrophied; in one case the aortic valves were calcified, and the left ventricle hypertrophied; in one case there was atheroma and insufficiency of the aortic and mitral valves, with hypertrophy of the left ventricle.

In the cases of simple mitral insufficiency: in one case the mitral valve was atheromatous and insufficient, there were old pericardial adhesions, and the left ventricle was dilated and hypertrophied; in one case the pulmonary and mitral valves were both thickened and insufficient, the left ventricle dilated and hypertrophied; in one case a large portion of the endocardium of the left ventricle was thick and white, the left ventricle dilated and hypertrophied; in one case both the ventricles were dilated and hypertrophied.

Of the two cases in which there was no hypertrophy: in one case the entire pericardium was adherent, the adhesions were calcified, and some of the calcified tissue had broken down into a thick, yellow fluid; in the other case the mitral valve was covered with vegetations, and there were antemortem thrombi in the left ventricle and right auricle.

Of the remaining 17 cases of cardiac disease without pigment induration of the lungs: 8 were cases of simple hypertrophy of the left ventricle; 4 were cases of dilatation of both ventricles; 5 were cases of thrombi in the different cavities of the heart.

In the other viscera, especially the liver and kidneys, lesions were usually found.

Lungs.—Thickening of the pulmonary pleura and old pleuritic adhesions were found in all the cases. A moderate amount of clear serum in the pleural cavities was found in 10 cases; œdema of the lung tissue occurred in 6 cases; hemorrhagic infarctions occurred 6 times, in 3 cases with mitral stenosis, in 1 case with aortic valvular vegetations, in 1

case with mitral insufficiency, in 1 case without valvular lesions; red hepatization, recognizable with the naked eye, was found in 4 cases.

No purulent or fibrinous exudations were found in the bronchi.

In the 40 cases of cardiac disease, without pigment induration, hemorrhagic infarctions occurred 9 times; congestion and œdema 16 times; emphysema and chronic bronchitis 15 times; croupous pneumonia 6 times; phthisis twice; no lesions 3 times.

Liver.—In 10 cases the liver had the so-called nutmeg appearance; small once; large 3 times; medium size 6 times; in 2 of the cases the intra-lobular veins were dilated. The nutmeg appearance was due to congestion of the centres of the acini, sometimes accompanied by a pigmentation of the liver cells. The consistence of all these livers was very firm. In 2 cases the liver was fatty; in 4 cases firm, and of medium size; in one case the liver was cirrhosed; in 3 cases it appeared normal.

In the cases of cardiac disease, without pigment induration, the liver was found normal 19 times; nutmeg 6 times; cirrhosed once; hepatitis gummosa 3 times; small twice.

Spleen.—In 3 cases there were infarctions in the spleen; in 3 cases the organ was of unusually large size; in the other cases nothing abnormal was noticed.

Kidneys.—In 16 of the cases the kidneys were the seat of chronic hyperæmia (stanungsniere); the kidney tissue usually congested, very hard, either of medium size (8 cases), or enlarged (6 cases), or atrophied (2 cases); the epithelium of the tubes of the cortex granular and broken down; the Malpighian bodies unchanged, except in the atrophied form. In 2 cases there were infarctions; in 1 case the kidneys were large, the cortex thick and of a grayish semi-translucent colour, the pyramids red, the Malpighian bodies waxy, and the epithelium of the cortex tubes granular.

Brain.—In one case of mitral stenosis there was an embolus in the right middle cerebral artery, and softening of brain tissue on the same side.

Symptoms.—The clinical histories are taken from the record books of the hospital, and are, for various reasons, imperfect, especially is this the case with the physical examination of the chest and of the urine. Only the positive evidence is of any value, the non-mention of any symptom does not exclude the possibility of its having been present.

In 6 cases there was a history of previous rheumatism; in 1 case, of scarlet fever. In 4 cases death was sudden. Dyspnœa was present in 11 cases; cough in 9 cases; hæmoptysis in 5 cases; angina pectoris in 1 case; œdema of the legs in 8 cases.

The urine contained albumen and casts in 3; albumen alone in 1 case.

The cardiac murmurs are stated to have been as follows: mitral stenosis

in 2 cases, a direct and regurgitant mitral murmur; in 1 case an aortic and a mitral systolic murmur; in 5 cases no record.

Aortic valvular disease with mitral insufficiency, in 1 case a double aortic and a double mitral murmur; in 1 case an aortic systolic and mitral systolic murmur; in 3 cases a double aortic and mitral systolic murmur; in 1 case no record.

Simple mitral insufficiency, in 2 cases a mitral systolic murmur; in 2 cases no record.

Of the lungs it is stated that there was dulness over both lower lobes in 2 cases; coarse and fine rales over both lungs in 6 cases.

From the study of these cases it may be seen that this lesion of the lungs is not a mere pathological curiosity, occurring but rarely, and of no clinical importance, but is, on the contrary, an ordinary result of valvular lesions of the heart, and of very great significance in determining the prognosis of cardiac disease.

It may occur in any case of old cardiac disease in which the current of blood from the lungs into the left auricle is obstructed, but is most frequent with mitral stenosis.

The most dangerous feature of the process appears to be the gradual filling up of the alveoli of the lungs with new cells, and the consequent exclusion of the air.

The liver and kidneys undergo changes parallel to those in the lungs. The liver is little or not at all enlarged; becomes very hard and firm; its veins, especially the intra-lobular branches, are dilated; the liver cells contain granules of brownish pigment. The kidneys also change their size but little; have an almost stony hardness; contain sometimes the same new pigment. Whether the vessels are dilated is uncertain, as no injections have been made. The epithelium of the tubes becomes granular, and breaks down.

The rational and physical symptoms, although not positive, are often such as to warrant a probable diagnosis. All the symptoms which indicate an obstruction to the passage of blood from the lungs into the left auricle, point in some measure to this disease. The existence of dyspnœa, cough with mucous expectoration and hæmoptysis, without any evidence of emphysema, are also evidences of pigment induration.

The physical signs are merely a general loss of resonance over both lungs, sometimes over circumscribed regions, frequently over the lower lobes, but then caused by hydrothorax. The respiratory murmur is feeble; there are coarse and subcrepitant rales varying in amount from day to day.

The name of pigment induration, given by Virchow to this condition of the lungs, I have preserved, not because it is entirely satisfactory, but to avoid introducing a new name when there is one already in use.

Pigment Induration of the Lungs.

No.	Sex	Age	Heart.	Lungs.	Liver.	Spleen.	Kidneys.	Brain.	History.
1	F.	..	Dilatation of right ventricle; stenosis and thickening of mitral valve; insufficiency and thickening of tricuspid valve.	Pigment induration; old pleuritic adhesions.	Congested medium	.....	Medium congested	.....	No history.
2	F.	28	Hypertrophy and dilatation of both ventricles; aortic valves rigid, mitral valves stenosed.	Double hydrothorax: old pleuritic adhesions: pigment induration.	Fatty	.....	C. P. N.[1] medium	.....	Mitral direct and regurgitant murmurs; general anasarca.
3	M.	19	Hypertrophy of right ventricle; sterosis and thickening of mitral valve.	Double hydrothorax; old pleuritic adhesions; pigment induration; numerous red infarctions.	Small nutmeg	.....	C. P. N. medium	.....	Rheumatism, dyspnœa, cough, direct and regurgitant mitral murmurs, râles over both lungs, dulness over both lower lobes.
4	F.	38	Hypertrophy and dilatation of left ventricle; large vegetations on aortic valves; mitral valve insufficient.	Double hydrothorax; old pleuritic adhesions; pigment induration; partly decolorized infarctions; fibrous induration of both apices.	Medium firm	.....	C. P. N. large	.....	Rheumatism, dyspnœa, cough, hæmoptysis, hectic, œdema of legs, urine contained albumen and casts, double aortic and double mitral murmurs, over both lungs coarse and fine râles.
5	M.	19	Old pericardial adhesions; hypertrophy; atheroma and insufficiency of mitral valve.	Old pleuritic adhesions; pigment induration.	Medium	.....	C. P. N. large	.....	Rheumatism, dyspnœa, angina pectoris, ascites, urine normal, mitral systolic murmur.
6	F.	46	Hypertrophy, thickening, and insufficiency of mitral and pulmonary valves.	Old pleuritic adhesions; pigment induration; excess of pigment.	Fatty	Large	C. P. N. large	.....	General anasarca, irregular heart action; sudden death.
7	M.	18	Hypertrophy and dilatation of left auricle and right ventricle; stenosis of mitral valve.	Old pleuritic adhesions; pigment induration; œdema.	Medium intralobular veins dilated	.....	C. P. N. medium	.....	Dyspnœa, general anasarca, casts and albumen in urine, aortic and mitral systolic murmurs.
8	M.	..	Hypertrophy and dilatation of right and left ventricles; stenosis and calcification of aortic valves; calcification of mitral valves.	Double hydrothorax; old pleuritic adhesions; pigment induration.	Large nutmeg	.....	C. P. N. large	.....	Died suddenly.
9	M.	22	Hypertrophy; atheroma of aortic and mitral valves.	Hydrothorax; old pleuritic adhesions; pigment induration; irregular red hepatization.	Medium	.....	C. P. N. large	.....	Rheumatism, dyspnœa, cough, and muco-purulent expectoration, coarse râles over both lungs, aortic and mitral systolic murmurs.
10	F.	..	Hypertrophy, thickening and stenosis of mitral valve.	Double hydrothorax; old pleuritic adhesions; œdema; pigment induration.	Nutmeg	.....	C. P. N. medium	.....	
11	F.	31	Hypertrophy; insufficiency of mitral valves; thickening of endocardium of left ventricle.	Double hydrothorax; old pleuritic adhesions; pigment induration; one infarction; œdema.	Nutmeg	.....	C. P. N. large, vessels waxy	.....	Cough, hæmoptysis, hectic, œdema of legs, albumen and casts in urine, mitral systolic murmur; sudden collapse and death.

[1] C. P. N. is an abbreviation for chronic parenchymatous nephritis. Medium, large, and small, indicate the size.

No.	Sex	Age	Heart.	Lungs.	Liver.	Spleen.	Kidneys.	Brain.	History.
12	M.	26	Hypertrophy; atheroma of aortic valves; atheroma and stenosis of mitral valves.	Old pleuritic adhesions; pigment induration.	.....	Infarction	C. P. N. medium	Embolus of right middle cerebral artery & softening of brain	Rheumatism, headache, vomiting, œdema of legs, hæmoptysis, convulsions, acute mania.
13	M.	45	Hypertrophy; atheroma of aortic valve, stenosis and thickening of mitral valve.	Old pleuritic adhesions; pigment induration; infarctions; irregular red hepatization.	Nutmeg	.....	C. P. N. large	.....	Cough, dyspnœa, hæmoptysis, mucous râles over both lungs, œdema of legs.
14	M.	12	Pericardium adherent by calcified false membranes, some of these have broken down into fluid; no hypertrophy.	Old pleuritic adhesions; pigment induration; œdema; infarctions.	Cirrhosis	Large	.....	.....	Symptoms of typhoid fever and bronchitis.
15	M.	..	Hypertrophy of left ventricle; calcification of aortic valves; mitral valves insufficient.	Double hydrothorax; old pleuritic adhesions; pigment induration.	Large nutmeg	Large	C. P. N. large, infarction	.....	Dyspnœa, general anasarca, double aortic and mitral systolic murmurs.
16	F.	26	Hypertrophy of left ventricle; dilatation of right ventricle; stenosis and thickening of mitral valves.	Old pleuritic adhesions; recent pleuritic exudation; pigment induration; infarctions.	Nutmeg	.....	C. P. N. medium	.....	Cough, hæmoptysis, dyspnœa, general anasarca, albumen in urine; died comatose.
17	F.	35	Hypertrophy; aortic valves have large vegetations; mitral valves atheromatous.	Double hydrothorax; old pleuritic adhesions; pigment induration; red hepatization of middle and lower lobes of right lung.	Large nutmeg	Infarction	Infarction	.....	Dyspnœa, cough, œdema of legs, no albumen or casts in urine, double aortic and mitral systolic murmurs.
18	F.	40	Hypertrophy and dilatation of right and left ventricles; aortic valves ulcerated and have vegetations; mitral valve thickened and insufficient.	Hydrothorax; pigment induration; irregular red hepatization; œdema; old pleuritic adhesions.	Intralobular veins dilated	.....	C. P. N. small	.....	Cough, dyspnœa, mucous sputa, œdema of legs, heart's action irregular, aortic double and mitral systolic murmurs, dulness over lower lobes of both lungs, coarse râles above.
19	F.	29	No hypertrophy; mitral valve has vegetations; in the left ventricle and right auricle are thrombi.	Pigment induration; œdema; old pleuritic adhesions.	.....	Infarction	C. P. N. medium	.....	Rheumatism, dyspnœa; died in an attack of dyspnœa.
20	M.	18	Hypertrophy and dilatation of right and left ventricles; mitral valves insufficient.	Pigment induration.	.....	.....	C. P. N. atrophied	.....	Scarlet fever followed by symptoms of Bright's disease, no heart murmurs.

ART. VI.—*Successful Reduction of Prolapsed Funis, by the Postural Method.* By ISAAC G. PORTER, M. D., of New London, Conn.

IN an extract from a foreign periodical, transferred to this Journal, (April, 1868, p. 550), in speaking of the knee-elbow position in prolapsed funis, the writer states that it was recommended by Deventer, in 1701; by John Mowbray, in 1724, and by Henry Bracken and others, in 1737, and adds: "When a loop of funis is still high in the cervical canal, and the cervix scarcely admits the examining finger, it may be that the knee-elbow position is useful; but when the loop has once passed through the os uteri, whether head, trunk, or foot present, it will be vain to expect any good from this position."

If the writer of the foregoing trusts, alone, to this postural method, without manual assistance, and *that* the knee-elbow and not the knee and chest position, his assertion is probably correct. The following case will show, however, not only the advantage of placing the chest and face and not the elbows on the bed, but also will tend to show the obligations due Prof. Thomas, of New York, who revived and modified the suggestions of Deventer and others, if he did not, as is probable, originate, *de novo*, the true method.

Mrs. S., æt. 27 years, the mother of three children, requested attendance at 9 P. M., Aug. 6, 1870, having been in moderate labour nearly all day; membranes had ruptured at 8 P. M. On examination, a portion of the funis, five inches in length, presented itself near the sacrum, the os uteri being more than half dilated. Placing her immediately on her knees, with her chest and face resting on the bed, efforts were made, between the pains, to return the cord beyond the head by introducing a large part of the hand. The prolapsus continuing to return on the removal of the support, the hand was suffered to remain *in situ* until expelled by the succeeding pain. Acting in this manner, while the head was accommodating itself to the passage, by making the "quarter turn," by which, in the large majority of cases, the occiput places itself squarely under the pubis, the effort was successful; the cord escaped from reach of the fingers, and, the head not receding between the pains, the patient was permitted to resume the usual obstetric position, and within an hour from entering the room, a good sized and healthy child was born. The funis was long and of small size.

Doubtless the operation is more feasible where, as in this case, the prolapsus occurred adjacent to the sacrum, for the obvious reason that, when the patient is in the assumed position on her breast and knees, and the funis is once replaced, it falls by its own weight towards the fundus uteri, over the face of the child, and lodges on its neck; a condition of things not so likely to happen when the cord escapes anteriorly and beneath the pubis, which, in the assumed position, is, of course, less elevated as regards the fundus uteri, than the sacrum; a distinction which it may be well to remember, when the question arises, whether assistance shall be rendered by "turning," or by the postural method and the hand combined.

ART. VII.—*Contributions to the Clinical History of Hyperæsthesia.* By F. PEYRE PORCHER, M. D., Charleston, S. C.

THE following cases are added to those reported by me in a paper in the *American Journal of the Medical Sciences* for July, 1869, in which I endeavoured to explain the nature of these cases, and to show that they were distinct from "neuralgia;" and also that they were not described with sufficient fulness in the ordinary text-books published in this country.

I wish again to invite attention, at least, to the marked frequency of "hyperæsthesiæ," and to show that they are principally found to affect the gastro-enteric region, as explained by myself, and referred to by Romberg and Handfield Jones, as a form partly dependent upon disturbances of the cœliac plexus. The causes, however, lie beyond this, and are found in the several antecedent circumstances which I have related in detail as characterizing each case, and which are generally similar in character.

The following case is also reported for the reason that the attacks were repeated three times at intervals of a few months; each was brought on by a similar train of causes; I had a full opportunity of studying them; they were uncomplicated by any other diseased condition, and recovery followed in each instance.

CASE I. Mr. S., a married man, extremely temperate in his habits, had been subject to attacks of mental depression, caused by losses in business. From these he apparently recovered, and when he came under my care his complexion was florid, and he presented to a superficial view no appearance of anæmia. In his efforts to support himself and family he used great exertions, rose very early and retired late to rest—being always urged by the cares of business, and undergoing constant anxiety.

This culminated in three separate attacks at intervals of two or three months, during which he complained of excessive and persistent pains, night and day, over the gastric and left hypochondriac spaces, which lasted from three to six days with undiminished severity. He had no heat of skin, no fever or excitement of the circulation. His tongue was scarcely furred, there was slight torpor of the bowels, though these were occasionally moved during the attacks. He had no headache, and his mind was perfectly clear, but with some anxiety respecting the gravity of his sickness.

This man gradually recovered from each attack under the use of poultices made with stramonium leaves freshly gathered, sinapisms, with morphia and bromide and iodide of potassium given internally. He also had an occasional laxative, and enemata were administered.

There was no periodicity or intermittency about his attacks, nor any evidence whatever of rheumatism, neuralgia, or any similar affection; they seemed to be simply and purely uncomplicated examples of hyperæsthesia (probably from irritation of the cœliac plexus), dependent upon and caused by an impoverished state of the blood, prolonged anxiety, depression, over-work, and mal-assimilation of food, as described in my previous paper

on these neuroses, and by Handfield Jones, in his work on "Nervous Disorders."

The subject of this sketch, by proper regimen, by the use of iron and quinia, and by regulating his mode of life, has recovered perfectly. He has only to repeat his previous course of living to fall back into a condition which will induce another exhibition of nerve disturbance. The sequence is, 1st. Anxiety, overwork, irregular hours; 2d. Defective nutrition; 3d. Anæmia and consequent nerve disorder, displaying itself in the form of hyperæsthesia, which may be designated as an erethism (ἐρέθισμὸς) or irritation of the nerves. As in chlorosis, marked pallor and loss of flesh are seldom found among the symptoms. Persons, whether male or female, may suffer greatly from anæmia or chlorosis, and yet present to the casual observer a fair appearance of health.

CASE II. *Hyperæsthesia; region of the loin.*—In this patient, although the pain was more severe than in the first, I do not regard the case as so marked a one, as it was not so entirely free from possible complications as the first.

J. B., a hard-working, temperate, man, aged 55, of active temperament, and upon whose mind and thoughts the cares of business were constantly resting heavily, came under my observation in two attacks of severe hyperæsthesia, with an interval of four months between them.

He had been using some exertion, stooping and lifting objects of no great weight, when he commenced to experience pain in the loin over the upper border of the crest of the ileum. This increased in intensity until it became so great that for three days he did not dare to turn in bed. He gradually recovered under the use of anodyne poultices, with morphia, chloroform, and potash, given internally. He had no catarrh, rheumatism, lumbago, constipation, fever, or neuralgia; nor were there any evidences of irritation of the kidneys or ureters. It is not likely that the pain resulted from the rupture of muscular or other fibres in the structures about the loin or hip, as the exertion which he used was prolonged but not violent. His nutrition and mode of living generally were not calculated to invigorate the health. He was, besides, dyspeptic, and presented evidences of anæmia.

If "hyperæsthesia" be only one development of nervous disturbance, or a species of the genus "neurosis," it is a true, constant, and well-marked species which, though exhibiting itself in various localities, will be frequently recognized; it should be more isolated and should have a specific name, and should not be permitted to remain merely as a symptom to be referred to only in treatises on general pathology. I have endeavoured to trace and describe more fully its true characters.

ART. VIII.—*Ostracism for Consumption.* By E. HOLDEN, M. D., of Newark, N. J.

THE following remarks are based upon personal observation at the principal resorts for consumptives both of this country and of Europe, and are not open to the objectionable bias peculiar to the invalid who has sought them for health.

It is lamentable that in an era which it is no exaggeration to call the era of banishment for consumptives, so little reliable information should be at hand, and the books, essays, and elaborate details that can be obtained are but too often the ventilation of pet theories, or the biased opinions of interested observers. The miserable experiences of Rio de Janeiro, and more recently of Iceland, are painful examples of the former, while in reference to the latter it might be invidious to particularize.

We read the dazzling descriptions of the shores of the Mediterranean, Madeira, Egypt, and Syria, on the one hand, and of Minnesota, New Mexico, California, or Florida on the other, and find the authors have vied with each other in drafts upon poetic imagery and the resources of language. All is *couleur de rose*, but the writer is rarely found to have visited any other than the locality praised, and the poor invalid, or too often his physician, decides in favour of the climate whose champion paints the most alluring picture.

Hundreds are said to recover; even the worst need not despair; thousands are made better, &c.; but the thousands who die are not mentioned. Even the wretched invalid himself, slowly and steadily running down to the dark river, nerved by contagious enthusiasm and with the well-known delusion so peculiar to the disease, often adds his most earnest though mistaken testimony. This is, moreover, singularly true of localities whose characteristics are the most diverse. From the snow-covered hills of Iceland to the scorching plains along the Nile, frigid, temperate, or torrid, we are persuaded to believe that each is the special Eden of rest and safety. We have in fact a parallel to the testimony in favour of any of the most prominent among the charlatans and empirics of the present day.

There are dozens of clairvoyants, astrologers, and pseudo-scientific men in the United States who have their crowds of believers, and concerning whom favourable testimony is circulated with as fervent assiduity as concerning any climate in the world, and but that the field is wider in the one case with always a grain of truth that the other does not possess, the parallel holds good.

There are two singular facts to be noticed in every resort for invalids: one, that the season is always a very unfavourable one, such as was never before noticed; and the other, that every one who fails to improve has come too late, a month or a year sooner would have been the thing, &c.

Another fact, and one that furnishes a commentary on the subject, is that, like the prophet who is not without honour save in his own country, the invalid's own or the neighbouring climate fails to show any particularly roseate hue, and he yearns for some distant Elysium.

There were last winter, during my stay at Mentone, two gentlemen from Minnesota for pulmonary troubles whose disease had been contracted in the most healthful portion of that certainly healthful State. The same thing is frequently noticed in Florida, and *vice versa* in Minnesota. The inference is a plain one, *not* that the reputation of these resorts is a purely fictitious one, but that however conducive to general health the influence of any particular climate may be, there will be found other special and potent influences to have been at work in the cases of recovery from phthisis. Still farther, that as in every other department of our profession, the remedy in question cannot be indiscriminately applied.

It is probable that the chief of errors in this matter lies in the fact that some specific curative property is believed generally to exist in the air or surroundings of any given locality, an error that is unhappily not confined to invalids alone. The next great error, and which follows almost as a corollary to the first, is, that there is anything or has ever yet been found anything which is actually a specific for the disease in question. That we may be clear, let it be understood that not the pseudo-inflammatory form is meant, which pathologists will probably yet unite in styling by some distinctive title, but tubercular phthisis, tuberculosis, consumption.

If we strip ruthlessly off the tempting garb that the cellular pathology of the day has thrown over observers and observations alike in regard to phthisis, we may well ask ourselves where lies the kernel in the gigantic growth—this result of years of scientific industry.

The very success that has followed the use of cod-liver oil and the phosphates, exhibits plainly the fact that we have known the disease by certain familiar characteristics only, that we have recognized a mal-nutrition, and instead of correcting this are able only to supply more readily assimilable material for nutrition—in other words, instead of bringing up the assimilative power to the healthful standard, we adapt its work to its weakness. Nor is this the less true whether we adopt the views of Virchow, of Niemeyer, of Williams, of Bennett, of Buhl, of MacCormac, or any of the numerous writers who have advocated peculiar views of the characteristic essence of the disease. And here may be well suggested the real effect of change of climate, viz:—

Having this unknown secret foe, and familiar only with its distinguishing features, a healthful locality may be assumed the chief essential to improvement, and even admitting that a locality may be worthy of the term healthful, if healthful to the largest number, yet the very word as applied to a climate includes for the diverse demands of civilized humanity the idea of *adaptation*, and in adaptation we have the key-note to its successful

application to phthisis. To be more explicit, every health resort originally selected as such for purity of air or water, freedom from sudden changes of temperature, moisture, &c., has without doubt certain distinguishing characteristics, such as a stimulating action upon the nervous system, or the opposite, tendency to produce or relieve hepatic or portal torpor, or persistent sedation of integumentary circulation, or the opposite, &c., and in each locality these characteristics are at once either beneficial or the reverse to the sensitive consumptive, however inert in their influence upon the sound.

Unfortunately there often exists familiarity with but one phase of climate in this particular; an invalid exhibits a constant tendency to pulmonary hyperæmia and hæmoptysis, and it needs but little science to determine that he would be unlikely to improve in a climate where the whole integument is kept blanched and bloodless by a low temperature at the expense of engorged viscera, yet even of such cases numerous examples are not wanting at health resorts, and often unfortunately through the mistaken judgment, to call it by no worse name, of physicians. Having then pure air and a climate adapted to necessities, we remove the chief obstacles to the assumption of nature's mysterious and always curative sway, while a proper supply of assimilable elements of nutrition arrests or rather balances retrograde metamorphosis—the consumption of tissue.

If we accept this view, and fairly acknowledge that our present pathology, although contributing to far greater success than any of the past, yet only approximates the truth, we do not exclude the hope that the discovery of the real essence of tuberculosis, the "Reiz," the condition, the fault, which decides the "origo" of this "commencing decay" (or as Dr. Southey in his work on the "Nature and Affinities of Tubercle" expresses it by the apt quotation

> "The rift within the lute
> That by and by will make the music mute"),

and which is the missing element in the admirable pathology of the age, is not far away.

To return to the matter in hand, let us analyze any number of cases of convalescence in three or four of the chief resorts for pulmonary disease, *e. g.*, the open sea, Florida, Minnesota, and the South of France, and what are the elements—freedom from customary care and anxiety—change of scene, variety, amusement, and relaxation—a strong confidence and hope of improvement—exercise without fatigue, freedom from changes of temperature, or the combined deleterious effects of heat and moisture, and chiefly pure air—and yet with these albeit invariable requisites alone, only a small proportion recover.

One-half or two-thirds as is asserted of Minnesota is assuredly an exaggeration, and in almost every melancholy failure that has come under

my own observation the essential element has been overlooked, viz., *adaptation* to idiosyncrasy of individual.

In all resorts a common remark of invalids who find themselves either at a standstill or becoming worse, is, "I never did feel well in this kind of climate, and, though beneficial to some, it doesn't suit me." Farther than this, those whose means and strength have permitted, who have gone from one resort to another find, if not made worse by exhausting travel, that some one of the places promoted improvement more than any other; with what an increased chance of success would the invalid have been banished from home if the home-physician had been able to decide upon the proper resort at the outset. Everywhere there may be found invalids who shiver over their scanty fires or languish beneath exhausting heat, whose own personal experience is outraged in their ostracism, and who would if they had been but asked the most simple questions as to individual peculiarities and needs, have been saved bitter disappointment and regret. It is, however, by no means meant to be assumed always an easy matter for a physician to advise judiciously on so serious a subject, and a moment's reflection should convince that it is a grave duty both to seize upon every opportunity for qualifying one's self to give such advice and to use every means to withstand the current of opinion that leads so many to banish invalids from home without consideration. It requires but to ask the basis of this popular opinion to ascertain its weakness. Only those who recover spread the praises of the locality where they found health, and two-thirds at least of these had no tubercular disease at all, but broken by overwork and anxiety, they could not but improve under the beneficial influences already enumerated. The numberless invalids in their sick-rooms at your hotel are as overlooked as they are silent, and the ceaseless tide pouring homeward to die are as voiceless in testimony as the dead who have preceded them.

The tendency toward this therapeutic ostracism gathers volume with every year, and the man who adds a syllable in favour, unless qualified as advice should be in regard to every other therapeutic agent, only places himself in the reprehensible position of those who vaunt their syrups or cordials or golden pills as panaceas.

Aside from a marked diversity of influence as regards localities remote from each other, famed climates possess intrinsic diversities usually dependent upon geographical subdivisions, *e. g.*, in Florida the delightful temperature of Indian River, under the direct influence of breezes from the Gulf Stream, often dry and stimulating, differs materially from the perpetual summer warmth of Enterprise, or the atmosphere of Jacksonville, St. Augustine, or Magnolia, albeit this very peculiarity of the climate of Florida is one of its excellencies, the localities named being accessible from each other, and opportunity for change and variety comparatively easy. The malarious influences, moreover, differ in intensity in different parts. The same is true of Mentone, San Remo, and Nice. At the former place

a vast amphitheatre made by a ridge of the Maritime Alps on the north, east, and west, with the Mediterranean on the south, is divided into two by a promontory on which is built the old town, and as a consequence the eastern curve is the most sheltered from cold northerly winds, and is somewhat less liable to change of temperature. Nice, too, once famous throughout Europe for pulmonary ills, has a climate ranging in a remarkable degree both as to its influence in remote sections of the town, and as to the precise month of the year at which a visit is likely to be made. Romantically fashionable, it is often windy and dusty, and becomes in the spring, a most dangerous resort. The climate is well styled stimulating, for surely nothing can be more so to a diseased and sensitive pulmonary tissue, than clouds of lime-dust aided by chilling winds from the northeast, or the mistral from the mountains of the northwest. To speak thus from personal experience corroborated by testimony of numerous individuals, invalid and otherwise, taken on the spot, of a climate once lauded as almost that of Paradise, will perhaps both serve as the example intended, and emphasize what has been said in reference to thoroughly understanding the peculiarities of the locality we recommend.

Is it, however, possible to secure the adaptation which has been so dwelt upon, and which necessitates the study of our patient in a new light? My own conviction is that it is more than possible, and that the banishment of consumptives may yet be reduced to a science productive of marvellous success.

The open sea, or, better stated, a long sea-voyage, has been placed first among the few resorts enumerated. Testimony in support of such preference could be adduced to fill volumes, and the voices of many of the eminent observers of the day are united in respect to it. An experience of several years at sea, in addition to observation among those journeying back and forth from their winter retreats, enables me to speak somewhat positively on this head.

It is singularly true that the influence of a sea-voyage is *more generally* successful in improving the condition of consumptives than that of any individual climate, nor does it seem of great importance whether the voyage be through warm or cold latitudes, provided of course the excess of the Tropics and Arctics be avoided; and the trite observation among sailors, that "you never can take cold at sea," is, although not universally true, significant of an extended observation grown to a proverb.

There are, undoubtedly, many exceptions to the beneficial influences of a sea-voyage, growing out of removable causes, such, for example, as undue exposure on deck, imperfect protection of feet and limbs, too short a voyage, and especially voyaging only within the influences of coast breezes. The latter statement will probably explain to many minds the numerous cases that appear exceptions to the beneficial influence of sea-air. There are but few consumptives who feel better near the sea-coast,

whether on land or sea, yet the very persons who grow ill and weak or suffer immediately from derangement of the bowels in such localities, feel speedy improvement when once out of green water and fairly off soundings. Even the coasts of Italy and Sicily, of Syria and Africa, afford no exception to this rule, and the apparent exception of the southern coast of France is only apparent since the frequent breeze from the ocean, that is so delightful and beneficial in that locality, is but a deflected land breeze that has crossed the mountain tops and strikes the sea about a mile from land. It is indeed the chief objection to a sea-voyage that a guantlet has to be run involving no little hazard; the delay in harbor, the necessity often for keeping close in shore, and the frequency of chilling winds in such localities, expose the invalid to inflammatory exacerbations or complications that the most favourable subsequent influences may fail to overcome. The two objections usually urged against a sea-voyage, viz., seasickness, and ennui, are almost always exaggerated, since, with scarcely an exception, both are self-limiting and rarely last more than a few days. The first in fact seems really beneficial, and is followed by a keenness of appetite to which the patient may have been long a stranger. The question is moreover frequently asked, "How about my tendency to hemorrhage? Will I not bring it on by retching and vomiting?" After careful and extended inquiry, I have become convinced that the answer may be safely and positively, "No"—and an interesting case recurs to me of an assistant surgeon in the navy (Dr. R.) who exhibited on shore a marked tendency to hæmoptysis, but who, though always a victim to sea-sickness, not only had no such trouble at sea, but experienced great relief from distressing dyspnœa. He suffered also from cough only when on soundings.

Still, although the depression of pulse that accompanies nausea may even be a safeguard against hæmoptysis, yet it cannot be denied that there may be exceptional cases of great excess, in which straining with ineffectual effort to vomit may induce hemorrhage.

In respect to ennui, however, it may be observed that while the monotony of a sea-voyage is for a while almost intolerable, yet, when not accompanied by home-sickness, it soon becomes to the invalid the most beneficial and luxurious rest.

It is, of course, unnecessary to add, that in every banishment from home it would be madness for an invalid to expect improvement if without the companionship of near friends or relatives; home-sickness and loneliness being but too surely the foes of convalescence.

By those who seem to have a lingering faith in the actual curative properties of climate, attempt has been made to ascribe the chief benefit at some localities to a saline condition of atmosphere. Experiments have been somewhat recently made, in the Genoese Riviera, to establish this property in the breezes that sweep shoreward from the Mediterranean, and the fact that blue sea-water owes its depth of colour to excess of salt seems to

lend plausibility to the idea. The success of the experiments implied by both Drs. Bennett and Lee has not, however, been attained in my hands, even with the most sensitive plates, *except* during a breeze sufficient to load the air with actual spray, and then only at a comparatively short distance from the shore, and rarely on the habitable level.

At sea, however, it is very common to find the air thus laden, and a tub of fresh water exposed on deck in any breeze will, within twenty-four hours, show sufficient salt to the test of nitrate of silver to give a cloud of the chloride. That such atmosphere at proper temperature should be directly beneficial to a diseased membrane, accords with daily experience in treatment of nasal or bronchial catarrh by means of Thudichum's apparatus or the steam atomizer.

This property of sea-air, moreover, accompanied by purity, a freedom from customary cares, the almost impossibility of fatigue, together with the other adjuvants to convalescence already mentioned, seems to offer in the highest degree to certain classes of consumptives the hope of relief. Yet there are, perhaps, many for whom such resort is not available, *e. g.*, those who have not the means to go with every comfort and the companionship of near friends, those who suffer from home-sickness to a great degree, but especially those who suffer from rheumatic or inflammatory complications. Those whose disease is far advanced are not added, for the most ordinary judgment enjoins that the banishment of a dying invalid to any quarter or locality whatever be made only if there be a full consciousness of the danger and *a complete resignation to death, however soon it may come*, fully recognizing the smallness of the prospect of benefit; and this exception is but a just one, for many an invalid has been carried on board ship (and I speak from personal knowledge) in an apparently helpless condition and lived long to bless the hand and voice that dictated the change. It is, in fact, too often but cruel mockery to say "Oh, you should have gone earlier before your disease was so far advanced," thus ascribing to delay alone a failure due only to a miserable mistake.

In what respect, indeed, does the selection of a retreat for an invalid differ from that of any other remedy? Does not the difference in success between one physician and another often lie in the judicious selection of remedies, and not in an outline knowledge of mere general properties, and are age and experience more than synonyms of skill in their adaptation! There may be found in every resort for consumptives, those who after weeks of trial are at least not improved, yet who linger hopefully after the stimulus of novelty has subsided, being precisely, in fact, in the condition of the patient who having received one prescription from a physician persists in its use long after common sense and the continuance of the disease should have dictated farther counsel.

But to return: the efficacy of a long sea-voyage is gradually becoming

more evident, and in the climatic treatment of phthisis is most deservedly assuming its rank among the curative measures within our reach. The testimony even of those who have simply crossed the Atlantic on their way to Italy, Madeira, or Syria will be almost universally found in its favour.

Within a week after losing sight of land, the cough frequently subsides or changes in character, dyspnœa is relieved, the appetite returns, and with it a sense of new vigour and hope of recovery.

Many thus benefited have prolonged their voyage, and the cheap rates of passage on sailing ships with their maximum of comfort, and their desirable length of voyage, are offering favourable opportunities within the reach of purses not particularly long. The voyage to Cape Horn, Sydney, and thence to San Francisco by sail, offers to those brave enough to endure it, all the advantages of mild climate, novelty, variety, and indeed all the combined benefits to be looked for in a sea-voyage, and this route is already resorted to by sufferers.

It may be interesting in this connection to refer to a prevalent opinion among consumptives who try the sea, that shows how current beliefs may usually be found based upon proximate though distorted truth, *e. g.*, there is usually a dread lest they may *not* be sea-sick, as all who escape are supposed to be too far gone to recover, whereas, the truth is, simply that consumptives are less liable than others to the annoyance.[1]

As the object of this article has been simply that of a protest against the growing recklessness in sending patients who fail to improve out of sight, and to assert and maintain that a gratifying success may yet be attained by studying consumptives from a different standpoint, any attempt to indicate precise rules applicable to successful discrimination of cases may most properly be referred to a future occasion.

With reference, however, to resorts in the Eastern Hemisphere, nothing of general applicability could be added to the admirable chapter on change of climate, in the work of Prof. Walshe on diseases of the lungs.

As to American resorts, however, with which foreign authors seem so little familiar, it may be briefly stated that Florida comes correctly under Group II. in the work referred to (with Madeira and Palermo).

Aiken, South Carolina, under Group IV., since more than any other American locality it approaches the climate of the Genoese Riviera, while Minnesota, finding no counterpart in the long list, might form a supplementary addition to Group VI. (Places of which the favourable influence is mainly dependent on their altitude above the sea level) a clearer defini-

[1] A number of cases of patients and former friends we e prepared as bearing upon this portion of the subject, but are omitted in the belief that what is above written will find ample corroboration in the observation of every one whose experience extends even no farther than a voyage at the right season across the Atlantic.

tion for this individual locality being, however, that of a climate essentially dry and cool, with keen stimulant quality upon the *vascular* system more or less predominant, in contradistinction to that of Mentone, which so markedly stimulates the nervous system.

In reviewing the whole subject, there is one reflection that cannot but force itself upon us, viz., that after all, change of climate, invaluable and promising as it is, as a curative measure for consumption, must yet fall far short of general applicability, since its successful prescription implies *means*, and means in abundance.

It has already been said that it would be madness for a patient to leave home without near and dear friends, but without ample means it is suicide, and worldly and paradoxical as it may seem, there has been no truth more strongly forced upon me than that—life, like every other blessing, is purchasable with money.

ART. IX.—*Case of Fibrous Stricture of the Rectum relieved by Incisions and Elastic Pressure; with Remarks.* By WM. R. WHITEHEAD, M.D., of New York, Physician for Diseases of Women, at the Northwestern Dispensary, Fellow of the New York Academy of Medicine, etc. (With three wood-cuts.)

THE following case of stricture of the rectum was operated upon recently, at my clinic at the Northwestern Dispensary, and a somewhat novel and painless method of dilatation was adopted after the incisions. It has proved to be so effectual that possibly the case may be thought worth recording, together with a few remarks on stricture of the rectum, the pathology of which does not seem to have been generally well understood, even by those who have written special treatises on diseases of the rectum.

Mrs. P., æt. 38, was married nine years ago, and has been a widow since eight years; no children; one miscarriage at the fourth month; she has never been well since. A year after her marriage she noticed about the labia and anus some sores which were long in healing, and which appeared successively one after the other. She frequently examined herself with a mirror. The sores finally healed, after persisting several months, and at one time she saw at the anus a crack, which was excessively painful, especially while at stool. Not long after the sores had healed, she was troubled with diarrhœa, and a muco-purulent discharge from the anus. During the last eighteen months this discharge was quite puriform, and at times escaped involuntarily, and compelled her to wear a napkin to prevent it from soiling her garments. Occasionally she had constipation with pain and flatulent distension of the abdomen, succeeded by diarrhœa and a more copious discharge of pus from the rectum. Her husband once had some trouble about his testes. Mrs. P. presented in her own history no evidences of constitutional syphilis. There were no eruptions on the skin

or about the throat. Defecation was usually difficult rather than painful, and the feces were flattened and about the thickness and width of a tape-worm.

The woman was pale, much emaciated, and had an exceedingly despondent look. There were hypertrophic masses of skin about the anus. The rectal touch revealed the presence of a very tight stricture just above the sphincter ani muscle, and about two inches from the anus. The first joint of the index finger could only partially enter the stricture, causing great pain. As measured with the little finger, the stricture was conical, and about three-quarters of an inch in length. On different days several attempts at dilatation were ineffectually made, causing much pain. On the 20th of August the patient was etherized, placed in the "semi-prone" position, and the large blade of Sims' duck-bill speculum was hooked into the posterior segment of the sphincter ani muscle, which was forcibly retracted, and the speculum confided to an aid. The anterior part of the anus was depressed with the handle of a pair of scissors. This dilatation of the sphincter ani muscle gave an excellent view of the stricture, which was tightly closed, and the shining fibrous borders of its lower orifice presented only a transverse linear depression. I cut the stricture, deeply and thoroughly, in the middle line, both posteriorly and in front, with a pair of stout, straight, and sharp-pointed scissors. The large blade of Sims' speculum, which measures $1\frac{5}{8}$ of an inch in width, was readily moved past the stricture. The hemorrhage which followed the very free incisions was moderately profuse for a while, but soon ceased after some clots of blood were removed from the rectum and bits of ice applied to the bleeding surface. Every precaution was previously taken to avoid excessive hemorrhage, and the incisions were conducted with a due regard to the thickness of the recto-vaginal septum. The rectum was largely dilated so as to afford a good view of nearly the whole of its interior. This satisfactory exploration of the rectum was effected by the aid of Sims' speculum, which was held by my clinical assistant, Dr. Francis M. Deems, so as to very forcibly retract the posterior part of the sphincter muscle. A sponge about the size of a small orange, and secured to a string, was pushed up the rectum as high as the sigmoid flexure, and by distending the gut, aided the exploration, and at the same time afforded security against an undesirable "débâcle" from above. The following suppository was left in the rectum: ℞.—Pulv. opii gr. iss; ext. belladon. gr. $\frac{1}{3}$; olei theobromæ q. s. Some brandy and a glass of milk-punch were given to the patient. She was left on the operating table a few hours, and afterwards walked home with the aid of her sister.

August 21. Slept well during the night; she had one very large evacuation from her bowels, followed by several smaller ones. Ordered beef-tea and milk-punch.

22*d*. 11 o'clock A. M. She felt chilly early this morning; slightly feverish; appetite impaired; diarrhœa. Passed No. 11 rectal bougie, then a bivalvé speculum, the blades of which were separated $1\frac{3}{4}$ inch at their extremities. *This dilatation caused considerable pain, and gave me some apprehension.* Tinct. cinch. co. ℥ss, ter die.

23*d*. 12.30 P. M. Skin hot and dry; pulse 160 and small; no appetite, tongue dry, great thirst; diarrhœa; complains of internal heat. ℞.—Morphiæ sulphatis gr. ij; spt. æther. nitros. ℥ss; aquæ ℥iss.—M. Teaspoonful every three hours. The rectum was syringed out with tepid water containing carbolic acid in the proportion of one drachm of acid

to a quart of water. Within four hours after the first injection the pulse fell to 120; within ten minutes after the injection the patient felt much relieved. Beef-tea and milk-punch continued. Injections repeated.

24th. 11.30 A. M. Skin moist; pulse 108; appetite improving. During the afternoon the pulse was 100. Condition very favourable. Carbolized injections in the rectum, washing it out thoroughly several times during the day.

25th. Diarrhœa profuse. R.—Pulv. Doveri gr. xv; bismuthi subnitratis ʒj; divided into six powders; one after each movement of the bowels. Milk-punch and beef-tea continued.

Sept. 6. She came to the dispensary and had a No. 12 rectal bougie passed into the rectum. The bougie was afterwards passed somewhat irregularly, and sometimes caused considerable pain, followed by fever.

I was much dissatisfied with the ordinary rectal bougies in use by surgeons, and was apprehensive lest I should perforate the soft and ulcerated mucous membrane above the stricture. This accident has occurred, and is liable to occur again, in the hands of even the most experienced surgeons, who persist in using the rectal bougie.

I thought that possibly Barnes' dilators might be used for keeping the stricture dilated, but they were found to be too stiff, and unsuitable for the rectum. I knew that Bushe had used in the rectum a pig's bladder distended with water, for stopping hemorrhage from this gut after operations on it. I improvised an apparatus, which I have since much improved, and which consisted originally of what is commonly known as a "condom" or "capote" attached to the short pipe of Davidson's syringe, and provided with a stopcock. At first I used gut condoms, but have since substituted for them rubber capotes, which are better. They dilate the stricture without pain when slowly distended with warm water, and are tolerated by the rectum for hours without discomfort. If, however, the sphincter ani muscle is too much pressed on laterally by the lower part of the distended capote, there will be an effort to expel it. To obviate this occurrence, the pipe of the apparatus which is embraced by the sphincter should be at least one inch and a quarter in length. Besides there should be on the outside a smooth rubber flange to prevent the pipe from being drawn too far into the rectum. The accompanying wood-cut, Fig. 1, illustrates the instrument which I use, and which was made for me by Messrs. Tiemann & Co., of New York. It is of hard rubber and surmounted by a delicate and flexible hard rubber probe, not much stouter than Dr. Chas. A. Budd's uterine probe, four inches long, and to which the capote is attached superiorly. The probe is screwed into the pipe at its bulbous end, and may be replaced by others which are longer or more or less flexible.

Fig. 1.

Oct. 10. The health of Mrs. P. has improved in a most remarkable manner; she has large natural passages without pain or difficulty; she has gained much

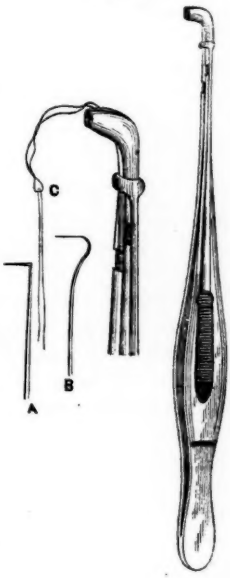
flesh, is quite cheerful, and profuse with her expressions of gratitude. The elastic dilatation is used about three times a week.

It may not be amiss to refer to a few peculiarities of the lower part of the rectum, and which have a direct and practical relation to stricture of that organ, as well as to operations which are practised on the lower portion of this intestine. The lower third of the rectum corresponds to a considerable distension of this gut, and which has been called the ampulla of the rectum, from its supposed resemblance to a round flask. We may for convenience of description compare the sphincter part of the rectum to the neck of this flask; in fact this part of the gut is a kind of muscular tube, the inner surface of which is lined with an acutely sensitive mucous membrane. On the living subject the length of the sphincter ani muscle is from an inch and a quarter to an inch and a half, and at its upper part there are concentric and puckered folds of the mucous membrane, and which folds mark the junction of the ampulla of the rectum with its sphincter portion. It is at this part that stricture of the rectum nearly always occurs as the result of syphilitic ulceration, and always, with very rare exceptions, in women.

The lower third of the rectum is usually not covered with peritoneum. Anteriorly this part is in relation in the male with the trigone vesicalis, the seminal vesicles, prostate gland, and membranous portion of the urethra, and quite exceptionally, with the peritoneum, which, according to Cruveilhier, may sometimes descend as low as the prostate. In the female, the anterior lower third of the rectum corresponds to the recto-vaginal septum. Posteriorly the rectum in its lower third is in relation with the hollow of the sacrum, the coccyx, and posterior fibres of the pubo-coccygeus muscle (levator ani), which by their contractions constrict the rectum. When it is largely dilated with Sims's speculum its posterior wall is seen to be closely applied to the hollow of the sacrum. Laterally the lower third of the rectum is in relation with adipose tissue which fills the ischio-rectal fossæ, which are traversed by the inferior hæmorrhoidal arteries. Incisions in the median line on the anterior or posterior part of the rectum, one or two inches above the sphincter ani muscle, are not attended with much danger from hemorrhage. But should it occur, it may be controlled more readily than is perhaps generally supposed. At least it is desirable to know what are our resources in the way of little expedients for stopping hemorrhage from this part. The rectum can be examined and a good view of it obtained with Sims' speculum, and without rupture of the sphincter ani muscle, if not too suddenly and violently stretched. In fact, I am disposed to regard all other specula for the exploration of the rectum as far inferior to Sims' instrument. I believe that Foveaux's adjustment may add much to its usefulness; at least I have experimented with a view to

fix my ideas on this point, and think favourably of this adjustment.[1] Stuffing a sponge up the rectum greatly facilitates a rectal exploration, and is an ingenious device which I first saw Dr. Emmet use, but originating which, I believe, is due to Dr. Sims. When the interior of the rectum is freely brought into view in the manner mentioned, there is no difficulty in applying the persulphate of iron or the actual cautery with great precision to a bleeding part; or the ligature may be applied quite high up the gut. There is an easy way to tie a bleeding artery in a deep and narrow cavity, and which I have never had occasion to resort to in the rectum, but in other parts of the body. An inspection of the following diagram (Figs. 2 and 3) will help to explain the little device to which I allude, and which is so simple that my excuse for calling attention to it is that I have seen eminent surgeons sometimes quite puzzled and fail to ligate a bleeding artery for want of such a device.

Fig. 2.



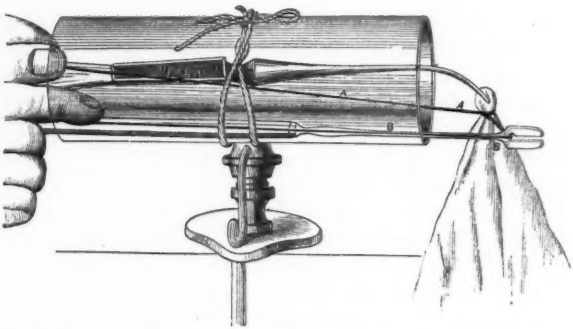
The part to be constricted is hooked up with a long and slender but strong tenaculum, such as either of those represented in Fig. 2, A, B; or this part may be seized with a pair of spring-forceps. They should be carefully held by an aid, while the ligature is being passed and the knot tightened. For passing the thread, a small piece of silver wire doubled, and containing the thread in its loop, and held with Sims' forceps, may be conveniently used, as also shown in Fig. 2, C. The thread being passed, is arranged for making an ordinary knot. One part of the thread is then slipped through the forked portion of Sims' wire adjuster, which is held with the right hand. The thumb of this hand fixes one end of the thread by pressing it against the handle of the adjuster, while traction is made on the other end with the left hand. The knot is thus gradually slipped down, with the right hand to the part to be tied, the fork of the adjuster taking the place of the finger. A tighter knot can be tied in this manner than with any of the ordinary knot-tiers in use, and will be found serviceable for strangulating the pedicle of a small polypus in the rectum or uterus.

Fig. 3 was copied from a photograph, and is designed to give an accurate idea of this ligature device. For illustrating it, the chimney of the lamp of a laryngoscope was selected as most convenient, and was properly

[1] See Braithwaite's Retrospect, vol. 58, p. 226.

secured in front of a photographic apparatus, and the thread, instruments, and other parts concerned were photographed. A pair of spring-forceps, carried through the length of the chimney or glass tube, holds a pocket-

Fig. 3.



handkerchief in front of the distal orifice of the tube. The tense thread passing through the fork of the wire adjuster is shown by the letters A, A, and B, B.

In operating on the anterior wall of the lower third of the rectum, if the incisions are too deep, there is some danger of cutting the peritoneum. The distance of the peritoneum above the anus, as mentioned by Lisfranc and Blandin,[1] offers considerable variation. As regards women, I believe that even slight prolapsus of the uterus will in some measure explain this variation. It is well known that in displacements of this organ upward by pregnancy or uterine fibroids, the peritoneum abandons a part of the anterior surface of the rectum, as it does in the male during distension of the bladder. A partial descent of the uterus from relaxation of the utero-sacral ligaments or relaxed vagina in dragging down the peritoneum, places this membrane in danger of being wounded or perforated by the knife. A simple way to determine the relations of the peritoneum to the lower and anterior surface of the rectum in such cases, is to make with the index-finger of each hand a conjoined rectal and vaginal examination, to determine the position of the utero-rectal *cul-de-sac* formed by the peritoneum which covers the utero-sacral ligaments.

Stricture of the rectum may be the result of cancer, or of the proliferation of histological elements which do not undergo such a retrogressive change.[2] The disease may also be the result of congenital or acquired

[1] Dict. de Méd. en trente volumes, art. *Rectum*.

[2] See Bouisson, Vices de Conformation de l'Anus et du Rectum. Thèse de Concours. Paris, 1851, p. 22. See also in the thesis of Dr. Jose Ricardo Sauri, of Mexico, a report of Richet's case. Thèses de Paris, 1868. Rétrécissements Fibreux du Rectum.

valvular obstruction of the rectum, but most frequently is the consequence of a peculiar kind of ulceration of syphilitic origin. This last variety of stricture of the rectum occurs just above the sphincter ani muscle, and almost invariably in women who have had soft chancres about the vulva and anus. Owing to the peculiar anatomical topography of the ano-genital region of the female, the chancrous matter of syphilitic sores infects this region, occasioning rhagades or fissures of the anus. There is an extension of the syphilitic ulceration to the ampulla of the rectum, where it remains long after the healing of the external sores. In certain lymphatic and debilitated women the ulceration sometimes takes on a phagedenic character, and, when cicatrized, causes a tight fibrous stricture.

The difficulty which has usually attended the study of stricture of the rectum has resulted from authors confounding in a single description all kinds of stricture of this intestine. Bushe mentions "ulceration of the rectum which may arise from the direct application of a venereal poison, or consecutive to disease of the genital organs, and coexisting with other secondary symptoms."[1] It was from analogical reasoning that Bushe inferred the existence of syphilitic ulceration of the rectum as a secondary manifestation, and it would indeed seem difficult to explain why the mucous membrane of the lower part of the alimentary canal should be entirely exempt from a secondary symptom which is so frequently met with in the upper part. He makes no mention, however, of the connection of syphilis with stricture of the rectum, although his descriptions point clearly to cases which were due to this cause. Quain,[2] who has written a practical and well-considered work on diseases of the rectum, does not mention syphilitic ulceration as a cause of stricture of the rectum. He speaks of cicatrization of an ulcer as a cause of stricture, and reports a case, but it is manifestly typical of syphilitic ulceration; and he reports another case under the head of idiopathic stricture which is quite as characteristic of the same kind of ulceration. Curling, I believe, mentions the connection of syphilis with stricture of the rectum. Ashton[3] does not allude to this connection. But Henry Smith[4] distinctly expresses the belief that stricture of the rectum is produced either directly by the specific ulceration of the part affected, or by contact of the discharge from the surrounding parts.

Huguier[5] described a peculiar ulceration of the vulvo-anal region followed by hypertrophy of the vulva. His plates illustrate very well this form of

[1] A Treatise on the Malformations, Injuries, and Diseases of the Rectum and Anus, New York, 1837, p. 143.

[2] Diseases of the Rectum, 2d edition, 1855, pp. 184–191.

[3] Diseases of the Rectum and Anus, American edition, 1865.

[4] The Surgery of the Rectum, London, 1868, p. 41.

[5] Mémoires de l'Académie Nationale de Médecine, 1849: L'Esthiomène ou Dartre Rongeante de la Région Vulvo-Anale, p. 501.

syphilitic disease, and in a few cases he points out its connection with stricture of the rectum.

M. Gosselin,[1] a remarkably accurate observer and highly competent syphilographer, has very much enlarged our knowledge of stricture of the rectum. He reports twelve cases, all occurring in females, and expresses the conviction that the disease is local and purely the result of a cicatrization, and that it is not constitutional. He thinks, however, that the peculiar hypertrophic enlargement of the vulva in certain cases is due to the long-continued irritation caused by the presence of soft chancres. He believes that this irritation will also account for the thickening of the submucous cellular tissue at the seat of the ulceration which causes the stricture. I think that the puckered folds around the internal orifice of the anus peculiarly favour the cicatrization of this part when ulcerated.

M. Gosselin's arguments against the opinion that the disease is a constitutional effect of syphilis seem conclusive. It is well known, he observes, that stricture associated with syphilis very rarely occurs in men; if the disease were constitutional, there would be no reason why it should occur nearly always in women. It is equally well known that stricture of the rectum rarely, if ever, yields to an anti-syphilitic treatment, while other secondary symptoms yield to this treatment. Tertiary symptoms affect the osteo-fibrous tissues principally, and no "*tumeur gommeuse*" has been found in the muscular tissue at the seat of a stricture of the rectum. Gosselin saw stricture of the rectum in two of his patients who had primary sores on their entrance into his syphilitic wards. One patient was under his observation eight months, and the other ten months; they were not, and never had been, subjected to a course of mercury, and no constitutional symptoms made their appearance, yet each of these patients had stricture of the rectum.

Després[2] points out very clearly the relation between phagedenic chancres of the rectum and stricture of this intestine. He reports cases which exhibited, while under his observation at the Hôpital de Lourcine at Paris, the progress of the disease from the time of the appearance of soft chancres about the vulva and anus to the complete development by cicatrization of a fibrous stricture near the junction of the ampulla with the sphincter portion of the rectum. Soft chancres are often seen about the vulva and anus, and syphilitic ulceration of the rectum is not extremely rare. In certain debilitated conditions of the system, the soft chancres take on a phagedenic appearance, and are sometimes very destructive, perforating the recto-vaginal septum, or destroying the labia and lower part of the vagina; and as the ulceration spreads internally, it heals externally.[3]

[1] Arch. Gén. de Méd., vol. v., 5ème série, 1854, p. 666.

[2] Arch. Gén. de Médecine, March, 1868: Des Chancres Phagédéniques du Rectum.

[3] See Fig. 2, Plate 3, of Huguier's Memoir, op. cit.

Usually, according to Desprès, phagedenic ulceration of the rectum is not deep.

Fissures and fistulæ in ano and in perineo, or fistulous tracts opening into the labia, sometimes complicate stricture of the rectum, and condylomata about the anus are frequent. The stricture may be partly fibrous, tight, and unyielding, the fibrous constriction being from half an inch to two-thirds of an inch in extent; or the stricture, while forming, may give by the rectal touch the sensation of irregular elevations or of crescentic folds which readily break down from pressure of the finger. The intestine sometimes four or five inches above the stricture is ulcerated, and the extensively ulcerated surface is terminated superiorly by a festooned border. This surface secretes constantly a large quantity of pus, which mixes with the fecal discharges. The permanent contraction of the sphincter ani muscle favours the accumulation of this pus in the ampulla of the rectum, and thus perpetuates the ulceration. It lasts for months, and years sometimes, before the formation of a stricture, and afterwards the mucous membrane above the stricture continues to be ulcerated. This mucous membrane is softened, and so much ulcerated that in certain cases it may be perforated by the incautious use of a bougie which may enter the peritoneal cavity. Rectal fistulæ occasionally form by the ulceration of the mucous membrane above the stricture.

We should suspect the presence of stricture of the rectum when there is a constant puriform discharge from the anus, and accompanied with occasional constipation and dyspeptic symptoms, and frequent diarrhœa. The patient is most commonly a woman, and between seventeen and forty years of age. Often she is pale, has a troubled look, and complains sometimes of itching about the ano-genital region. She probably recollects to have had a painful ulcer of the anus, and, on examination, there is found to be condylomata around the anus, and the rectal touch detects a stricture of the gut about two inches from the anal orifice. Defecation is painful while the ulceration occupies the sphincter portion of the rectum. When the ulceration is healed at this part, there is usually no pain, though there may be a very profuse secretion of pus from a largely ulcerated surface above. When the stricture is cancerous, it is usually situated higher up the rectum, and may be attended with sharp pain and general symptoms of cancer.

In stricture, the consequence of syphilitic ulceration of the rectum, there is often a long-continued and exhausting suppuration, which debilitates the patient and predisposes to phthisis. The treatment by the usual method of dilatation with bougies is generally considered palliative rather than curative, and is not entirely devoid of danger. I believe that dilatation with the rubber capote will obviate this danger. One capote is sufficient; but, if desired, two or three may be used, one over the other. The capote may be distended to the capacity of the largest bladder; and if introduced

into the rectum and filled with ice-water, would answer better for stopping hemorrhage from the rectum than a pig's bladder; though I have already called attention to the advantages of a thorough exploration of the rectum with Sims' speculum to aid in stopping hemorrhage from this part. I can see no reason why the rubber capote, properly fitted to a suitable tube, may not be used in dilating a stricture of the œsophagus. I suggest, incidentally, this manner of treating stricture of the œsophagus as worthy of a trial.

To return to our subject, an anti-syphilitic treatment of stricture of the rectum is worse than useless. This treatment does harm by increasing the debility of the patient. When the stricture is fibrous, as in the typical case which I have reported in this article, it should be very thoroughly divided with the knife or scissors, and kept properly dilated. There should also be made to the diseased mucous membrane above the stricture suitable applications, such as the chloride of zinc or carbolic acid sufficiently diluted.

149 WEST TWENTY-THIRD ST., NEW YORK,
October 24, 1870.

ART. X.—*On Immunity from the Action of Morbific Agents.*

By D. FRANCIS CONDIE, M. D., of Philadelphia.

A CAREFUL study of the human organism during a state of health will, we think, convince every one that it is endowed with an inherent power which enables it to resist the action of all such agents as are capable of disturbing the regular performance of its several functions. Although this conservative power has, it is true, from divers causes, become to a considerable extent impaired, we, nevertheless, in every community meet with individuals who enjoy in a very great degree an immunity from the action of morbific agents generally, to the influence of which all the other members of the community are alike susceptible.

The fact just stated was recognized by the leading medical authorities of an early date, though it has been in a great measure overlooked by those of a later period. Still, every observing physician, conversant with the diseases of malarious and otherwise unhealthy localities, where every portion of the population is equally exposed to the same unwholesome atmosphere, is aware that many of the inhabitants of such localities live, even to an advanced age, in the enjoyment of a good degree of health; escaping entirely an attack of the diseases which prevail there endemically.

The same thing is true in respect to nearly all zymotic diseases. During their prevalence, some members of the community, without adopting any special precautionary measures, escape an attack.

The almost entire exemption of the negro from yellow fever in places to which the disease is indigenous, is a well-established fact. To the attack even of diseases which are recognized as being among those possessed of the most marked contagious character, some persons enjoy an entire immunity, no matter how complete their exposure. Thus, we have known an individual, employed as a nurse in one of the wards of a hospital devoted to the treatment of chronic diseases of the skin, and whose duty it was to wash and anoint, twice every day, patients affected with psora, in its most virulent form, and yet to escape entirely infection, with no other precaution being taken by him than simple ablution of his hands with soap and water each time his task was completed.

There are individuals who enjoy a similar immunity from the poisonous effects which result from contact with the juices of or the emanations from certain noxious plants, which can never come in contact with the skin of other persons without producing more or less intense inflammation of an erysipelatous character. We have known persons who could handle with perfect impunity the poison vine (*rhus radicans*) as well indeed as any of the other species of poisonous sumach, while others, again, dared not approach within a few feet to the windward of the plant without experiencing its poisonous effects.

That the cases in which immunity from the action of morbific agents, to which reference has been made, are not to be received as instances of merely accidental or casual exemption from the influence of prevailing health disturbing agencies, is proved by the fact that similar immunity is manifested in all subsequent instances of similar exposure.

It is true that, occasionally, we meet with cases in which the resistance to morbific influences was, at first complete, but which became, after repeated exposure to such influences, at short intervals, or even, sometimes, as it would seem, simply with the lapse of time, more or less impaired, if not entirely lost.

In not a few instances the immunity from an attack of endemic or epidemic disease is manifested even in cases where the morbific matter is introduced into the system by direct inoculation. In our note-book several cases are recorded, in which primary vaccination was performed, with pure, recent, active matter; but without the operation being followed by any other result than a slight soreness at the part where the virus was introduced, and which was entirely gone in a day or two. The same thing occurred after a second and third insertion of vaccine matter, that had been used successfully in other cases; finally, inoculation with variolous matter was resorted to, and with the same negative result. In three of these cases the patients, after the lapse of six, eight, and fourteen years respectively, were exposed to variolous infection during severe epidemics of smallpox, and all contracted the disease, but in a very mild and greatly modified form.

The fact that such resistance to the action of morbific agents is pos-

sessed, to a greater or less degree, by certain individuals, must not be lost sight of in making up our estimate of the extent and value of the protection against variolous disease afforded by vaccination. Among every given number of persons who have been vaccinated, apparently with complete success, there will always be found some, and the average number will amount perhaps to some sixty-seven per cent., who will be endowed with complete and permanent protection from an attack of smallpox, even in a mild and modified form; others, in whom the protection, will be for a time complete, and continue so for a longer or shorter period in different cases, when the protection will become gradually more or less impaired, and finally fail entirely; while, in other cases, there will be, at all times, when exposure to the infection of variola takes place, a liability to an attack of the latter though in nearly every instance in a decidedly modified form—mild in its symptoms, short in its duration, and terminating neither in deformity nor in death.

Excepting in those cases, which, however, are of rare occurrence, where pure vaccine matter, recent and active, is employed, and where the operation is followed by neither constitutional nor local effect, it is impossible to judge, with anything like an approach towards certainty, the extent to which, and the period of time during which a vaccinated subject will enjoy exemption from variolous infection; the extent and endurance of the exemption having no direct relation to the character and course of the local phenomena, which follow the insertion into the arm of vaccine virus.

If now we are correct in our premises, which are based upon a very large series of careful observations of our own and of a host of other practitioners in this country and in Europe, the importance, nay, the absolute necessity of revaccination in all cases, to insure the complete prophylaxis furnished by the vaccine infection, against smallpox, is rendered apparent.

When smallpox occurs in the midst of a community of the vaccinated who are exposed to its infection there will always be some who are liable to an attack of the disease, which will be in nearly every instance more or less modified in its leading features—in its duration, in its severity, in its sequelæ, and in its mortality—in different cases; but which, even in this modified form, will be capable of giving rise to an infection or contagion by which the disease may be propagated, and in its most malignant form, throughout a large portion of the community. Now, having no certain means of determining to what extent they who have been already subjected to vaccination are completely protected from the action of the variolous infection to which they are liable to be exposed at any moment, it must be evident that to insure them from an attack of smallpox, and thus prevent its spread beyond the original focus of contagion, it is necessary, with as little delay as possible, to subject the entire community to vaccination, even those members who are reputed to have already successfully undergone the operation those, also, in whom the operation has been repeated even three or more

times. By this course in more than one locality in the interior of Pennsylvania the spread of smallpox among the indwellers, when introduced from without, has for several years past been promptly and effectually checked. In the same manner, the city of Providence, Rhode Island, has been completely protected from smallpox for the last fourteen years, as is shown by the annual reports of Dr. E. M. Snow, Superintendent of Health for that city.

In a large manufacturing establishment located within twenty-five miles of Philadelphia, one hundred and eighty hands, upon an average, are constantly employed, consisting of males and females in nearly equal proportion, and of individuals of different ages, from fifteen to sixty years; the great majority of the males being below forty-five years, and of the females below thirty. In this establishment the more or less frequent accession of new hands to supply vacancies produced by various causes, or to meet the increased demand for labourers, causes a frequently recurring liability to the introduction among the hands of smallpox. To guard against such an occurrence, the physician who has charge of the hands employed in this manufactory has, since the year 1859, subjected them all, indiscriminately, every twelve months, to revaccination. With the results of these revaccinations, in one hundred of the hands, including only such as had remained in the factory continuously for the preceding ten years, we have been furnished. They are as follows:—

The 1st year's revaccinations gave apparently regular results in 20 cases; partial results in 8 cases; no results in 71 cases.

" 2d year's gave 16 apparently regular results; in 6 partial results; no results in 78 cases.

" 3d year's gave 13 cases apparently regular; in 10 partial results; no results in 77 cases.

" 4th year's gave 8 cases apparently regular; in 6 partial results; no results in 86 cases.

" 5th year's gave 6 cases apparently regular; in 3 partial results; no results in 91 cases.

" 6th year's gave 5 cases apparently regular; in 2 partial results; no results in 93 cases.

" 7th year's gave 3 cases apparently regular; in two partial results; no results in 95 cases.

" 8th year's gave 3 cases apparently regular; in 2 partial results; no results in 95 cases.

" 9th year's gave 1 case apparently regular; in 2 partial results; no results in 97 cases.

" 10th year's gave *no* case apparently regular; *one* irregular; while in 99 cases there were no results.

No case of smallpox occurred among the hands in the factory during the

ten years referred to, notwithstanding the prevalence of the disease on two different occasions within that time amid the community, of which these hands constituted a part.

The foregoing results of a series of revaccinations repeated annually for a period of ten years, are of a more interesting character than those based upon the ample series of revaccinations that have been for a long series of years practised among the soldiers of the standing armies of Prussia, Wurtemberg, and Bavaria, inasmuch as the subjects of the first series were the same throughout the entire period, while the latter series were performed on masses of men, to which new recruits were being constantly added every year.

In the foregoing paper we have taken for granted that, so far as our remarks bear upon the subject of vaccination, and the extent and duration of its efficiency as a protection against an attack of smallpox, the operation was in all cases skilfully performed, with genuine effective matter, while nothing occurred to interfere with its regular course. Our only object has been to show that the necessity for prompt and general vaccination and revaccination in every community into which smallpox has been or is in danger of being introduced, arises simply from the fact that amid every community there will be individuals insusceptible to that complete and permanent impression of the vaccine influence necessary to render it a full and enduring prophylaxis against an attack of smallpox; we have taken no account whatever of any of those accidental and avoidable causes by which vaccination is prevented from conferring upon its subjects a full and enduring protection from variolous infection; such as the use of spurious or effete matter, or when after a perfect vesicle has been formed it is early ruptured, thus causing a discharge of its contents; or by the production, by any means, of phlegmonous inflammation, and the formation of purulent matter in place of the generation of vaccine lymph, etc. etc.

We may remark, in conclusion, that among the hands in the manufactory where the revaccinations were performed, of the results of which we have given an account, the arms of several of both sexes who were of European birth, presented two, and even three well-marked vaccine cicatrices. Now in these it was found that only one per cent. of the revaccinations were followed by the slightest result.

ART. XI.—*Contribution to the Knowledge of the Physiological Action of Atropia.* No. 1. *Its influence on Pigeons.* By H. C. WOOD, Jr., M. D., Professor of Botany in the University of Pennsylvania.

ALTHOUGH there are very few drugs whose physiological action has been studied within the last fifteen years as closely as that of belladonna, yet

it does not seem that the time has come when the stamp of finished can be set upon our knowledge of the medicine. Wishing some time since to make a physiological comparison between atropia and morphia, I found it impossible to do so satisfactorily, and finally concluded to attempt to go over the whole subject, repeating all the hitherto performed experiments, trying to winnow the wheat from the chaff and, if possible, to discover new grains of truth. Whether this plan will ever fully be completed time alone can determine, but certain results have been developed already, which seem to me worthy of immediate publication, especially with the hope that they will elicit criticism, that in the fires of controversy may at last be fashioned perfected knowledge.

The first of these points, and the one to which the present paper will be confined, is in regard to the action of belladonna and its alkaloids upon birds.

In a prize paper published in the *Archives Générale*—, M. Lemattre confirms the assertions originally made by T. Wharton Jones (*Med. Times and Gaz.*, 1857) that atropia will not dilate the pupils of birds, stating that it is because their irides contain no radiating fibres, belladonna acting on these muscles in the higher animals.

The anatomical assertion here made rests chiefly, I believe, on the researches of Mr. Jones, but I think there can be no doubt of its falsity. The general laws of homologies throw great doubt upon it, and recent anatomists have most abundantly demonstrated the existence of such fibres. In the *Archives für Ophthalm.*, vol. 15, part I., is a beautifully illustrated paper by Alex. Irvanoff and Alex. Rollett, clearly showing their position and power, and in Max Schultze's *Archives für Mikroscop. Anatom.*, vol. 6, Heft I., 1870, is a paper by Johann Diegel, essentially corroborating the former. The anatomical explanation of Jones and Lemattre seems, therefore, to be erroneous, and the following experiments were made to test their physiological assertion.

Expt. 1.—Adult pigeon. Oct. 26, 9.20 P. M., dropped very freely three successive times a solution of sulphate of atropia in eye (gr. iij to ʒj), and held head so that solution remained in a little puddle for several minutes; 9.27 no dilatation of pupil; 9.50 there has been no dilatation of pupil.

Expt. 2.—Oct. 27. 2.30 P. M. Put 4 grs. ext. of belladonna in throat of pigeon used in first experiment, which it swallowed. 3. P. M. No visible effects from belladonna. 7 P. M. Gave by mouth 10 grs. ext. of belladonna. 9 P. M. Have examined pigeon several times; no effect from belladonna on pupil or otherwise visible. 10 P. M. There certainly has been no vomiting up to this time; no effect visible from belladonna. Oct. 27. The pigeon perfectly lively; no signs of having vomited. Oct. 28, 2.30 P. M. Gave 20 grs. of ext. of belladonna. 2.50 P. M. Pigeon reeling. 5.45 P. M. Pigeon has been running about perfectly lively ever since last entry. Gave gr. x extract of stramonium. The pupils are, I think, somewhat dilated. 6.30 P. M. Pigeon still unaffected. 10 P. M. Pupils are perfectly movable, contracting freely when brought to a light, but I think not so much so as normally. No other sign whatever of a mydriatic having been taken noticeable. No quietness or restlessness, or drooping of feathers, or apparent disordered vision. Oct. 31.

Pigeon perfectly well; gave gr. jss atropiæ sulph. about 1.15 P. M., in pills of bread. 1.50 P. M. Pigeon been running about perfectly lively and eating since; no perceptible dilatation of pupil. 6.30 P. M. Assistant states that the pigeon has appeared a little stupefied during afternoon; now shows no symptoms, except that pupils are somewhat dilated; they are a little irregular and apparently do not contract as much as they ought under a strong light.

Expt. 3.—Oct. 31, 1870. Three adult pigeons were given each, by mouth, 30 grains of fresh juice extract of belladonna at 3.20 P. M. 3.30 P. M. Pigeons walking about unconcernedly. 3.50. One of the pigeons has vomited some, but evidently only a portion of the belladonna. 4.15. Two pigeons have been vomiting since; one freely the other slightly. I do not know whether one of them was the pigeon that vomited before or not. 4.30. Pigeons vomiting. Nov. 1. Pigeons do not seem different from usual.

Expt. 4.—Nov. 3. 3 P. M. Gave two of the same pigeons, each 12 grs. of fresh juice belladonna extract, prepared by Ransom, of England. 5.30 P. M. Have not vomited; no symptoms from belladonna; gave each 12 grains additional. 7.30 P. M. There has been no vomiting; no effect apparent from belladonna. Gave 12 grains more to each bird. 10.30 P. M. Has been no vomiting; no effect visible from drug, save that there appears to be some slight dilatation of the pupil. Gave one pigeon 12 grains additional, the other 6 grains. Nov. 4. Pigeons perfectly lively this morning. The belladonna has not had any appreciable effect in disturbing their equanimity. There has evidently been vomiting during the night.

Expt. 5.—Nov. 4. An adult pigeon, one used in expt. 3, but not used in last, at 12.30 P. M. was given, by mouth, 10 grains of extract of stramonium. 1 P. M. No vomiting; pigeon not affected. 2.30. P. M. No vomiting; gave 10 grs. more extract of stramonium. 3 P. M. No vomiting or other perceptible disturbance. 7 P. M. No vomiting since; no perceptible effect from stramonium.

Expt. 6.—Nov. 5. 8.20 P. M. Gave an adult pigeon 3 grains of atropiæ sulph. made into pill with bread crumbs. 9.30. P. M. Has not vomited; does not seem even quieted. 10 P. M. Has not vomited or shown any effect of the atropia. 10.20. P. M. Seemingly unaffected; let loose in yard. Nov. 6. 9 A. M. Pigeon alive; has been vomiting this morning. 2 P. M. Has been vomiting off and on all morning; will not eat, is quiet, but runs away when approached. Nov. 10. Has not been well since 5th; some days eating pretty well, other days refusing; more or less diarrhœa, especially latterly; died last night.

Expt. 7.—Nov. 4. 1.15 P. M. Gave to one of pigeons used in experiment of previous day (No. 4) 1½ grains atropiæ sulph. injected in cellular tissue of breast and thigh. A little was lost during operation, possibly as much as a ¼ grain immediate vomiting occurred. 1.40 P. M. Has been vomiting again; no other symptom save that he is disposed to be quiet in corners. 2.30 P. M. Quiet in corners; feathers look ruffled; has had very watery evacuations. 3 P. M. Allows himself to be picked up (is tame at any time) without moving, but walks well towards a corner when removed from it; is perfectly rational; no dilatation of pupil. 7 P. M. Pigeon seems well, but has some tendency to be quiet; no dilatation of pupils. Injected into breast 1½ grains more of sulphate of atropia. Two or three drops of fluid escaped, containing probably ⅛ grain of atropia. 10 P. M. No perceptible change; no dilatation of pupil; seems fairly lively; got out of basket himself and left so soon as lid was taken off. Nov. 5. All right.

Expt. 8.—Nov. 5. At 7.40 P. M. injected in three places between 12 and 14 grs. extract of stramonium, in watery solution, into cellular tissue of breast and thigh of adult pigeon. 8.20 P. M. Pigeon perfectly quiet. Pupils wide, irregular, not contracting beyond a certain limit in a strong light, 9.30 P. M. Pigeon a little quiet; more so, I think, than is due to its being night. 10 P. M. No apparent change; pigeon was let loose in yard. Nov. 6. Pigeon is a little lame, otherwise not very sick. Noon. Pigeon has refused to eat as yet. Nov. 7. Pigeon died this morning. Unfortunately assistant threw it away be-

fore I had seen it, but there can be no doubt its death was owing to local changes induced by the hypodermics, a large amount of water having been used to dissolve the extract.

Expt. 9.—Nov. 5. 2.45 P. M. Injected grains 3¾ atropiæ sulph. at three points, into cellular tissue of breast and both thighs of pigeon. 3 P. M. Pigeon lying on belly unable to rise up; when raised up it flaps its wings, but allows itself to be freely handled without resistance. When laid on side vainly tries to get on breast. Pupils contracted rather than dilated. Breathing regular, abdominal. 3.10 P. M. Heart's action irregular; lies on back perfectly quiet, except for convulsive twitchings; eyes closed; pupils decidedly small, but further contracting in a strong light. 3.22 P. M. Pigeon just dead.

Expt. 10.—Nov. 5. 9.25 P. M. Gave 2½ grains atropiæ sulph. in solution, injected in both thighs of adult pigeon. Temperature in rectum 108¾° F. 9.32 P. M. No effect as yet. 9.35 P. M. Had a violent convulsion. 9.36 P. M. Another one. 9.37 P. M. Laid on side, tries only feebly to roll itself on its belly. 9.40 P. M. Temp. 107¼°. Pigeon lies on back with no other motion than almost constant convulsive twitchings. 9.50 P. M. Temp. 106¼°. 9.55 P. M. Pigeon dead. Temp. 106°. *Autopsy* 10 minutes after death. Heart quiescent, engorged with dark blood, not moving when pricked with knife. Intestines: Peristaltic action lively. Brain not perceptibly injected.

The first of these experiments was directed to test the local action of belladonna upon the pupil of the pigeon, and I think showed very distinctly that this mydriatic, used locally, fails entirely to dilate the pupil of the pigeon. The solution used was a strong one (gr. iij to ʒj) and was very faithfully applied, but the iris did not respond.

The next experiments were directed to determine whether belladonna given internally would act on the eye or not, and they appear to confirm the assertion of Lemattre. The belladonna was given in the form of extract, and of the pure salts of the alkaloid, given both by the mouth and hypodermically, and in no case produced very decided dilatation or any immovability of the pupil. In some instances, it is true, there was some apparent enlargement of the latter, but the experiments were made in the evening, when I have noticed an apparent tendency in pigeons to have dilated pupils even in a lighted room, and in those cases where the drug was given freely enough to produce very marked toxic effects, no dilatation of pupil whatever was induced. Moreover, in experiment 7, there was decided contraction of the pupil. Dr. Jones' physiological fact then seems to be accurate, although his explanation appears to be founded in error. I will not attempt to assign any reason for it, because I cannot imagine any one sufficiently plausible to be at all worthy of credence. At the present I think we must content ourselves with the bare fact.

In the earliest of these experiments I was very much astonished at the enormous doses of belladonna borne by the pigeons, and the experiments were multiplied to test this apparent inactivity of the mydriatics. The result is certainly to establish the fact that belladonna, stramonium, and of course hyoscyamus, resemble morphia in their want of action on these birds. It appears to be almost impossible to immediately kill them with the extracts, given by the mouth. Doses of over 15 grains they generally threw up,

and in experiment 4, 48 grains of the best English fresh juice extract were given in a little over seven hours without any appreciable narcotic effect.

In experiment 6 three grains of a very beautiful and pure sulphate of atropia were given and retained, and yet did not appear to interfere materially with the well-being of the pigeon.

The sulphate of atropia was then tried hypodermically, and it was found, as with morphia, that although it takes enormous doses, yet it is very possible to kill the birds with the poison.

In experiment 7 thirteen grains of the extract of stramonium injected into the cellular tissue produced no direct appreciable disturbance, and in another case 3 grains of atropia ($\frac{1}{4}$ of a grain or so being lost) injected in two doses about six hours apart, induced no effects that could be called toxic. On the other hand, in experiment 9 two and one-third grains produced violent convulsions, ending in death in half an hour; but then the pigeon had been certainly weakened by previous experimentation, running through several days, and interfering very greatly with digestion. In experiment 8 the pigeon lived for a longer time after the injection of nearly 4 grains. From all the experiments I think it may be deduced that it takes not less than 2 grains of atropia, given hypodermically, to kill an ordinary robust adult pigeon. In man, the toxic action of such a dose would at least equal that of ten grains of morphia. The power of pigeons to resist belladonna has not before been pointed out that I am aware of, although Mr. Anderson, as quoted by Pereira, asserts that blackbirds eat the berries with impunity, whilst Reil, according to Prof. S. R. Percy, declares that "sharp-sighted birds" are exceedingly susceptible. It is well known that herbivora are affected but slightly by belladonna and stramonium, whilst carnivora, like the cat and dog, are more readily influenced. Every frequenter of large cities has probably seen goats feeding freely on stramonium plants. Every experimenter knows how difficult it is to kill rabbits with the alkaloid, even when used hypodermically, and Drs. Ogle and S. R. Percy have fed rabbits for nearly a week on belladonna leaves solely without injuring them. Dr. Percy has given four ounces of dried hyoscyamus leaves to a goat and one drachm of extract of hyoscyamus to a kid without producing any symptoms save dilatation of the pupils.

Remembering this and the assertion just alluded to of Dr. Reil, I have been desirous of testing whether the same general law was true of birds, but have been unable to procure a hawk, owl, or other carnivorous bird.

The causes of the non-action of atropia on pigeons seem to be three in number: First and chiefly—absolute obtuseness of the nerve centres to its action, an obtuseness, the cause of which we shall probably be able to explain just so soon as we know why atropia acts on the nerve centres of man. Second—very rapid elimination of the poison, for I have noticed that the conjoint urino-fecal discharge becomes watery and abundant under

the action of the drug. Third—when taken internally, comparatively slow absorption, so that the poison is removed from the blood by the kidneys, etc., as fast as it enters it.

ART. XII.—*The Antagonism of Poisons.* 1. *Antagonism of Morphia and Hydrocyanic Acid.* By JOHN J. REESE, M. D., Professor of Medical Jurisprudence and Toxicology in the University of Pennsylvania.

THE subject of the antagonism of poisons, whether real or supposed, is one that has excited more or less interest among both toxicologists and physiologists; nor indeed is it void of interest to the therapeutist, since it may become a question of the utmost moment to him, whether he may venture to combat the dangerous and often rapidly approaching fatal effects of some poisonous substance, by administering an heroic dose of some other equally energetic poison.

Considered in the abstract, there would certainly seem to be no reason why one medicinal or poisonous agent, acting upon the animal economy in some one specific physiological manner, should not be controlled and even neutralized by another agent equally potent, whose *modus operandi* is directly the opposite of the former. Why, for instance, the powerful tetanic, strychnia, which throws the muscles into such fearful spasms through irritation of the spinal marrow, should not be *antagonized* by a substance, the direct effect of which is to relax the muscular system—such as tobacco, bromide of potassium, or Calabar bean. Many other instances will readily occur to the physician, where toxic agents of very opposite physiological character might naturally be supposed to counteract each other's effects, in the animal system.

I do not, however, desire at present to argue this question on its abstract merits. My aim is rather to present the results of a number of carefully conducted experiments, which are, however, by no means yet completed, but which, so far as they go, possess a certain value in throwing light on this vexed question.

My experiments, so far as they have progressed, have reference to the following supposed antagonistic poisons: morphia and atropia, morphia and prussic acid, morphia and strychnia, morphia and aconitia, morphia and arsenic, strychnia and tobacco, strychnia and aconitia, strychnia and tincture of chloride of iron, strychnia and tincture of iodine, strychnia and Calabar bean, and atropia and Calabar bean.

All these several experiments should, in my judgment, be carefully repeated over again, and that more than once, before we would be authorized

to deduce any really positive conclusions in reference to this important subject. The medical journals of the day are, as we all well know, full of novel and astonishing cases of just this sort of experimentation, in which a conclusion has been prematurely seized by an over-ardent imagination, and one which, unfortunately, will not stand the test of a critical repetition by other hands.

These present experiments, then, I desire it to be understood, do not claim by any means to be exhaustive; they are rather the beginning of a more elaborate series, which I hope to be able soon to prosecute. In the mean time, however, I have thought that, imperfect as they are as to their number and detail, the profession might feel some interest in perusing them.

The first of these experiments that I shall now notice are those which refer to the antagonism of *morphia and prussic acid.* But in order to get a perfectly clear idea of this real or supposed antagonism, it will be necessary, first, to understand precisely what are the symptoms produced by each one of these poisons when taken separately. Then we shall be the better qualified to appreciate the effects resulting from their combination. I therefore select from my notes first, a few experiments which exhibit the effects of *prussic acid* when taken alone, and then some illustrations of the effects of *morphia* when given alone; after which, the results of administering the two poisons in combination will be detailed.

Expt. 1. *Prussic Acid alone.*—One fluidrachm of the ordinary prussic acid of the shops (2 per cent.) was put into the throat of a moderate-sized dog. In half a minute, the respiration became panting. In 1 minute, he fell over in convulsions; pupils dilated. In 3 minutes, there was retching; in 4 minutes, another slight spasm, with voiding of urine; in 5 minutes, attempts to get up; is unable to stand at the end of 8 minutes, but otherwise appears unaffected.

In 10 minutes after giving the first dose, a similar quantity was administered by the mouth. In half a minute, the respiration became rapid and noisy; in three-quarters of a minute, he fell over in a slight convulsion, uttering a faint cry. In 3 minutes, he lies perfectly quiet; pupils somewhat dilated; eyes apparently insensible. In 12 minutes, appears to be gradually dying; no convulsions. In 16 minutes seems more animated; eyes glazed. In 22 minutes, death takes place almost imperceptibly.

Remarks.—In this case, two fluidrachms of ordinary prussic acid, given in two doses, ten minutes apart, proved fatal in 32 minutes from the time the first dose was taken.

Expt. 2.—Thirty minims of ordinary prussic acid were injected hypodermically into a moderate-sized dog. In 7 minutes, no obvious effects were exhibited; the dose was now repeated. In 4 minutes after, slight vomiting took place. In 5 minutes, the gait became unsteady; no cry; appears as if partially narcotized. In 6 minutes cannot stand for any length of time; pupils natural. In 7 minutes, vomiting repeated, and more abundant. In 10 minutes, vomits bile; bladder and rectum evacuated. In 12 minutes, the peculiar spasmodic movement of the jaws, followed by more vomiting of bile; pupils somewhat dilated; seems to struggle against the spasms and gradually lies down. In 14 minutes, tenesmus. In 15 minutes, there is general rigidity, though he maintains the erect position; great tenesmus. In 16 minutes, there is a general

rigid spasm, though still on his feet. In 17 minutes, lies down, with his legs doubled under him. In 20 minutes, spasmodic action of the jaws, with retching and vomiting of bile. (Apparently, the force of the poison has been spent upon the alimentary canal.)

In 24 minutes, the dose was repeated (30 minims hypodermically). In 1½ minutes, the peculiar spasms were observed; falls over; but no cry; strives to keep upon his feet. In 3 minutes, he falls, in partial convulsions. In 4 minutes, the spasm is general, but no rigidity. In 8 minutes, violent spasm, which does not continue. In 9 minutes, slight rigidity. In 10 minutes, convulsions of fore legs; lies on his back. In 12 minutes, violent convulsions; kicking of the legs. In 30 minutes, the respiration decreasing in frequency, gasping; apparently dying. In 33 minutes, seems better; respiration more frequent and less laboured.

He was left in this condition; and the next morning, he was so well recovered as to be able to walk about, although evidently weakened.

Remarks.—In this case, one and a half fluidrachm was given, in three separate doses, all within 24 minutes, from the effects of which the animal barely recovered.

Expt. 3.—A small terrier bitch took about 25 minims of ordinary prussic acid, by the mouth. In half a minute, she reeled over in convulsions, uttering a slight yell. In 1 minute, respiration was very laboured, 60 per minute; pupils moderately dilated. In 4 minutes, slight rigidity. In 6 minutes, respiration was 32, gasping; lies perfectly quiet; pupils less dilated; slight rigidity in the fore legs. In 10 minutes, respiration was 30, very laborious; bowels evacuated; complete relaxation; pupils rather more dilated. In 15 minutes, respiration 24, more laboured. In 20 minutes, respiration 25; pupils less dilated; relaxation of muscles complete. In 25 minutes, respiration 40, stertorous; pupils about the same. In 30 minutes, respiration 44. In 33 minutes a transient convulsion. In 1 hour, respiration 40; slight rigidity of fore legs, along with a slight convulsion; after which the *cold affusion* was practised. No apparent benefit resulted at first, violent convulsions occurring 14 minutes subsequently, with very contracted pupils. In 20 minutes, the condition remained much the same; tongue lies flabby in the mouth, purplish underneath; respiration 40; pupils still contracted, though sensible to the light. In 40 minutes, the pupils were natural; respiration 40. The cold affusion was now repeated; the animal rouses under it; shakes herself, and screams out under its repetition. In 50 minutes, consciousness is evidently returning; the animal arouses and turns herself over. In 1 hour, the affusion was again repeated; she gets up, attempts to walk, dragging her legs; trembling of whole body; respiration 40, less laboured. In this condition she was left lying on her side. In two or three hours, she completely revived, appearing on the next morning, as bright as ever.

Remarks.—In this case, a comparatively small dose of prussic acid (25 minims) produced alarming effects, which appeared to be modified, if not counteracted, by the cold affusion, repeated.

Expt. 4. *Morphia alone.*—One-third of a grain of morphia in solution, was injected, hypodermically, into a young dog. In 1 minute, the pupils were much dilated. In 3 minutes, slight vomiting. In 5 minutes, slight moaning. In 7 minutes, some muscular debility; tends to lie down, but gets up again. In 15 minutes, pupils somewhat contracted; bowels evacuated; can walk about easily. In 20 minutes, appears drowsy, but very sensitive to the flies, etc. In 28 minutes, a similar dose was administered hypodermically; followed in half a minute, by whining. In 1 minute, there was general relaxation of the limbs, with a tendency to lie down. In 15 minutes, pupils contracted; lies down, but is easily aroused. In 20 minutes afterwards, he took another one-third of a grain by the mouth. In 3 minutes, there was dilatation of the pupils, with marked drowsiness. In 4 minutes, the eyes were closed. In 6 minutes, pupils

contracted, but sensible to light. In 10 minutes, seems fully asleep, but can be aroused. In 20 minutes, sleep more profound; breathing tranquil; can yet be partially aroused; coma never perfect. In 50 minutes, he has partially aroused; can walk about. He was left in this state, lying tranquilly on his side. In about three or four hours, he was completely recovered.

Remarks.—In this case, one grain of morphia was given to a very young dog, in three separate doses, all within forty minutes. The narcotic symptoms were all well pronounced, though the animal recovered in about four or five hours. In this, as well as in all my other experiments with morphia on dogs, the immediate effect was invariably to dilate the pupil; though the subsequent contraction was always well marked.

Expt. 5. *Prussic Acid and Morphia.*—Half a grain of sulph. morphia dissolved in water was injected hypodermically into a full-sized Scotch terrier dog, after a prolonged fast. In 4 minutes, he vomited mucus; some loss of muscular power; pupils rather dilated. In 11 minutes, the dose was repeated; followed by increased feebleness; pupils slightly contracted. In 17 minutes, 30 minims of the ordinary prussic acid were given by the mouth. In 2 minutes afterwards, he lies down; no cry, nor convulsion; pupils somewhat contracted; sensation perfect. In 13 minutes, fell over, and became comatose. In 45 minutes from the commencement of the experiment, he lay perfectly quiet; respiration 24; eyes very sensitive. In 1 hour, coma; respiration 19; difficult to arouse. In 1 hour 20 minutes, respiration 22; still very quiet. In 1 hour and 35 minutes, could be aroused; walked with a tottering gait. In 3 hours, he had comparatively recovered; responds to call; pupils natural.

At this time, administered half a grain of morphia and 15 minims of prussic acid combined, by the mouth. In half a minute, there was very rapid breathing; no cry nor convulsions. In 3 minutes, breathing very rapid and gasping; slight twitches of abdominal muscles. In 15 minutes, no visible effects except great debility; but he can yet partially rise upon his feet. Now, repeated the last dose, by the mouth. In half a minute, very rapid breathing. In 1 minute, fell over, in moderate convulsions; breathing rapid and stertorous. In 5 minutes, no yell; lies quiet; pupils moderately dilated. In 6 minutes, slight twitchings; rigidity increasing; pupils contracting. In 10 minutes, no rigidity; respiration very laboured. In 15 minutes perfectly quiet; coma; pupils contracted; respiration 18, labouring. He was left in this state, and at the end of 5 hours after the first experiment, he was so far recovered as to be able to run about the lot, from which he made his escape during the night.

Remarks.—In this case, a strong, full-grown dog took two grains of morphia, and one fluidrachm of ordinary prussic acid, in three divided doses, at intervals; from which he ultimately recovered. The symptoms observed and recorded evince a decided preponderance of the effects of the morphia over those of the prussic acid; which latter, however, was in a minimum quantity. In the succeeding experiment, these two poisons were given in more equal doses, and with a different result.

Expt. 6.—To a full-grown dog, though under average size, half a grain of morphia and thirty minims of ordinary prussic acid were given together by the mouth. In 1 minute, the gait was tottering. In 2 minutes, pupils much dilated; no cry; walks around as if dizzy. In 4½ minutes, vomits; loss of muscular power; sits down; no convulsion. In 15 minutes, repeated the same dose. In half a minute, the characteristic yell. In 1 minute convulsion, with very rapid and laborious breathing. In 2 minutes, used the cold affusion; pupils much dilated; respiration 56, stertorous. In 5 minutes, a strong convulsion; pupils less dilated. In 15 minutes, can stand erect; pupils still slightly dilated.

Now, again repeated the same dose. In half a minute, fell over; respiration

very rapid. In 1 minute, convulsion; no cry; pupils strongly dilated. In 1½ minute, a rigid spasm, followed by stertorous breathing. In 3 minutes, relaxation and less dilatation of pupils. In 4 minutes, respiration 36. In 10 minutes, only slight rigidity. In 30 minutes, no more convulsions; lies quiet, with stertorous breathing; pupils natural.

In 50 minutes after taking the third dose, it was again repeated—making in all two grains of morphia, and two fluidrachms of prussic acid given in the course of 1 hour and 20 minutes. In half a minute, the cry, and a violent spasm. In 2 minutes, the rigidity ceased; breathing very laboured and stertorous, only 12 per minute; pupils natural; complete coma and insensibility; respiration gradually became slower. In half an hour, respiration only 5 per minute; shortly after which death took place, in a convulsion.

Remarks.—This case would seem to show that, when morphia and prussic acid are given, each in a full dose, the symptoms of *both* toxic agents are clearly manifested. And further, that morphia does not counteract the fatal effects of prussic acid, unless it be in large excess over the latter—if it does so even then, but not if the latter be taken in a full poisonous dose. It may also be remarked that the cold affusion, which was only once employed in this case, appeared to be of no avail in counteracting the effects of the prussic acid, as in the former case (Expt. 5), possibly, because it was not followed up.

Expt. 7.—In this experiment the same quantity of morphia and prussic acid was administered as in the last-mentioned case (Expt. 6), viz., two grains of morphia, and two fluidrachms of prussic acid; and with the same fatal result.

One grain of sulphate of morphia was injected hypodermically into a large dog. In 3 minutes, there was slight dilatation of pupils. In 6 minutes, cries out. In 15 minutes, gave half a grain additional hypodermically. In 20 minutes, appears drowsy; lies down; pupils not yet contracted. In 30 minutes, slight contraction of pupils; sensibility perfect.

Now, gave by the mouth half a grain of morphia, with one fluidrachm of prussic acid; (the object having been to induce the influence of the slower poison before giving the more rapid one.) In 1 minute, no visible effect, except that the previous influence of the morphia was very apparent. In 2 minutes, relaxation perfect. In 3½ minutes, cries out; slight twitching of the fore legs. In 5 minutes, moans; pupils slightly dilated; breathing very rapid. In 6 minutes, apparent loss of sensibility. In 8 minutes, perfect relaxation; no convulsive action. In 9½ minutes, twitching of limbs. In 13 minutes, the eyes closed; pupils slightly dilated.

In 15 minutes, gave one fluidrachm of prussic acid, by the mouth. In half a minute, respiration becomes much slower. In three-fourths of a minute, groans and cries out. In 1 minute, bowels evacuated. In 1½ minute, a rigid convulsion, with great dilatation of pupil. In 2 minutes, relaxation; respiration rapid. In 3 minutes, convulsion, followed shortly by relaxation. In 4½ minutes, a slight spasm; rigidity of fore-legs. In 5 minutes, pupils dilated. In 6 minutes, spasm without rigidity. In 7 minutes, pupils contracting; spasm repeated. In 11 minutes, rigid convulsion; pupils more dilated. In 13 minutes, another spasm; respiration 46, stertorous. In 15 minutes, another spasm, which was repeated in 18 minutes.

In 1 hour, he lies quiet, in a deep sleep. In 1 hour and 10 minutes, a strong convulsion; no rigidity; eye natural. In 1 hour and 35 minutes, a severe convulsion, followed in 10 minutes by another one. In two hours, rigidity of the fore-legs; breathing stertorous; another severe spasm. In 2 hours and 20 minutes, a moderate convulsion. In 2 hours and 30 minutes, opisthotonos; respiration slow and gasping, soon ending in death.

Remarks.—In this case, as in the preceding one, the combined effects of the double poison were sufficiently well marked. The *fatal* result, in

both cases, is undoubtedly attributable to the prussic acid, rather than to the morphia.

The following experiment, in which the two poisons were used in rather smaller doses, though in about the same relative quantities, will illustrate how *nearly* fatal the results may be, without proving absolutely so. Here, the more active poison was the one first administered.

Expt. 8.—To a moderate-sized dog, full-grown, 30 minims of ordinary prussic acid were given by the mouth. In 1 minute, he fell over in convulsions. Almost immediately the cold affusion was applied to his head and chest. In 4 minutes, he vomited bile. In 5 minutes, was able to stand. In 8 minutes, the cold affusion was repeated; condition much improved.

In 10 minutes, he received half a grain of sulphate of morphia by subcutaneous injection, which produced moaning in 4 minutes, but no convulsions; pupils much dilated; is able to stand up. In 16 minutes, respiration 38. In 20 minutes, pupils less dilated; is very drowsy. In 25 minutes, respiration 28, regular; eyes blink; is perfectly quiet and drowsy.

In half an hour from the commencement, gave him fifteen minims, additional, of prussic acid. In 2½ minutes, groaning and restless. In 4 minutes, convulsions; bowels evacuated. Fell over before 5 minutes. In 6 minutes, still groaning; lies quiet; respiration 60; pupils as before. In 7 minutes, groaning repeated; gets up; respiration 36. In 10 minutes, respiration 40; groans on being disturbed.

In 15 minutes, took fifteen minims more of the acid. In half a minute, he began to twitch. In 1 minute, groans. In 3 minutes, slight twitching of the muscles of the face, followed by a general convulsion; pupils dilated. In 30 minutes, respiration 28, very laboured. In 40 minutes, eyes almost natural, and sensitive to light. In 1 hour, respiration 20, very laboured; lies perfectly comatose. A sudden severe spasm now took place, lasting nearly one minute; appears to be dying; pupils greatly dilated. He continued in this state for another hour, when he had a similar convulsion. In 2¼ hours, from the commencement there were occasional groanings; pupils dilated; appears somewhat better; breathing rather less laboured.

In 2 hours and 20 minutes, gave him one-third of a grain of morphia and fifteen minims prussic acid, by the mouth; deglutition very difficult. In 2½ minutes, the characteristic yell. In 3 minutes, slightly convulsed, followed by repose; respiration less laboured; is apparently insensible.

In 15 minutes, repeated the last dose; deglutition very difficult. In 2 minutes, the characteristic yell; prolonged convulsion. In 12 minutes, appears as if dying; the breathing becoming more prolonged and difficult; no convulsions. He was left in this state for dead, but, to my surprise, I found next morning, that he had revived during the night; and he appeared quite well, with the exception of some debility.

Remarks.—In this instance, as in the others, the combination of symptoms is sufficiently evident. The recovery of the animal, after such serious and protracted symptoms, seems remarkable. He took altogether one and a sixth grain of morphia, and one and a half fluidrachm of prussic acid, in divided doses.

All the above experiments are varied, as will be perceived, both as to the absolute and relative doses of the poisons employed, as well as to the mode of employing them. The deduction would seem warrantable, that the antagonism between prussic acid and morphia is very slight, if indeed it exists at all.

This fact assumes an importance by no means trifling, when regarded

in a medico-legal point of view. In a recent criminal case which has excited considerable interest in our community—that of Dr. Schœppe—it was strongly alleged by the prosecution that the deceased had died from having taken a combined dose of morphia and prussic acid, although the chemical evidence failed to establish the presence of either of these poisons after death; and the symptoms before death were utterly inconsistent with the existence of at least *one* of the alleged poisons, namely, prussic acid. The testimony most relied on by the prosecution, to sustain this very extraordinary allegation, was that of a physician, who undertook to say that the symptoms of the deceased (which were really only the ordinary symptoms of apoplexy) were such as would result from a combination of morphia and prussic acid, *because*, some thirty years previously, he (the physician) had administered to a chicken-hawk a mixture of corrosive sublimate, opium and prussic acid; and that the bird died the next day, after exhibiting certain symptoms which, in his opinion, resembled those of the deceased!

Medico-legal knowledge must, truly, be at a very low ebb, and its estimate by our courts of justice must be of an exceedingly trifling character, when they can permit so solemn a procedure as that of a trial for murder, to be disgraced by testimony so absurd as that above quoted.

But, inasmuch as murder *may* actually be committed by the administration of two or more poisons combined, under the impression that the usual well-known symptoms of each will be masked, or neutralized by the effects of the other, it becomes a matter of the utmost consequence to legal medicine, in addition to its interest to the toxicologist, the physiologist and the therapeutist, to arrive at some definite and positive conclusion as to the real facts of the case.

In a future communication, I shall offer some observations in reference to the antagonism between *morphia and atropia;* and, also, between *morphia and aconitia*, and between *morphia and strychnia.*

ART. XIII.—*Traumatic Paralysis of the Visual Accommodation.*

By GEO. C. HARLAN, M.D., Surgeon to Wills Ophthalmic Hospital.

A YOUNG man in full health came to the Wills Hospital, complaining of dimness of vision in the left eye. Fifteen hours before, the eye had been struck by a piece of iron chipped from a tool that he was working with. There was no pain, and no external appearance of injury but a slight general injection of the conjunctiva. He complained only of dimness of vision. No application had been made to the eye. The pupil was widely dilated, only a narrow rim of the iris to be seen. The acuteness of vision was very much diminished. At twenty feet, only No. 70 of Snellen's type

could be distinguished, instead of No. 20 as with the other eye. $V = \frac{2}{7}\frac{0}{0} = \frac{2}{7}$. Small type could not be distinguished at any distance. Examination with trial-glasses showed a hypermetropia of $\frac{1}{48}$, and with a convex glass of 48 inches focus No. 20 was read with ease at 20 feet—that is, his vision for distant objects became perfect. Vision for near objects was found to be equally perfect with glasses that accomplished the necessary accommodation for different distances. A few drops of a solution of the extract of Calabar bean were applied to the conjunctiva, and in twenty minutes the pupil was smaller than that of the other eye, and he could read the smallest type with ease, but not at a greater distance than eight inches, so that a *concave* glass was now necessary to restore his distant vision. The Calabar bean had not only rendered latent again the slight degree of hypermetropia and restored the power of accommodation, but, by excessive stimulation of the ciliary muscle, had made the eye temporarily myopic. I expected that it would be necessary to repeat the application, and directed the patient to return in two or three days, but he never presented himself again.

In an eye of perfectly normal refraction, the loss of the power of adjustment of focus would, of course, affect distant vision only. In this case a low degree of hypermetropia ($\frac{1}{48}$), which is easily neutralized by a slight unconscious action of the ciliary muscle, and never occasions inconvenience when the muscle is active, rendered the eye almost useless when the power of accommodation was lost.

Although many cases of paralysis of the accommodation have been recorded, as the result of various diseases, particularly of diphtheria, in some epidemics of which it has been quite a common accompaniment, it is rare as the result of accident, probably because the kind of injury necessary to cause it does not often fail to inflict, at the same time, some more serious mischief—as dislocation of the lens, intraocular hemorrhage, or separation of the retina. There is now under treatment, or rather under observation, at the Ophthalmic Hospital, a patient who was struck in the eye by an arrow, who presents the same symptoms—dimness of vision and a widely dilated pupil, with no other external sign of injury, and who can even read quite small print with the proper convex glass—but examination with the ophthalmoscope reveals a dislocated lens. One other case of uncomplicated traumatic paralysis of the accommodation has come to my notice. It was that of a medical man, a surgeon in the navy, who has given me the following account of it. While riding in a street-car one evening, a fellow-passenger, in attempting to close the sliding door, gave him a violent blow in the eye with the point of his elbow. There was acute pain at the time, but it soon subsided, and he slept as usual. On arising in the morning, he was horrified at finding that the power of near vision had gone from the eye, and, looking in the glass, discovered that the iris also had nearly disappeared. He went at once to a prominent oculist, who treated him with Calabar bean, and after several applications, repeated at intervals of two or three days, both the iris and the ciliary muscle regained their power.

ART. XIV.—*Memorandum of a Case of Re-amputation at the Hip, with Remarks on the Operation.* By GEORGE A. OTIS, M. D., Assistant Surgeon United States Army. (With three wood-cuts.)

IN a report published in 1867[1] I discussed the statistics of amputations at the hip for injury, and, after analyzing the reports of a large number of cases, referred[2] to the comparative safety of exarticulating the femur for diseased conditions succeeding a previous amputation in the continuity of the thigh. The history of the following case, with some additional evidence I shall adduce, will corroborate the opinion then expressed.

CASE.—Private Julius Fabry, Battery K, 4th U. S. Artillery, aged 38 years, was wounded on August 16, 1864, in an engagement at Deep Bottom, Virginia, by a shrapnel shot, which shattered the lower part of his left leg. The missile was moving with great velocity; and, after disembowelling the horse on which he was seated, exploded, and nearly tore away the rider's leg. The limb was amputated at four in the morning of August 17, at the field hospital of the Second Army Corps, by Surgeon G. W. Jackson, Fifty-third Pennsylvania Volunteers, by the circular method. The patient was sent to the Base hospital at City Point, and was thence conveyed to the Satterlee Hospital at Philadelphia, arriving there on August 20th. The conditions of the stump, and of the knee-joint, were such that it was deemed necessary to perform a second amputation through the joint. The hospital record states that this operation was performed on August 23, by Acting Assistant Surgeons W. F. Atlee and Egan. On August 28 the stump was suppurating freely. At this date a piece of copper from a fuse-screw was discovered in the muscles on the exterior of the thigh, and extracted through an incision about two inches below the great trochanter, by Acting Surgeon J. B. Roe. The patient was positive that a leaden ball was extracted from the calf of the leg at the time of the first amputation. This ball and the fragment of fuse-screw are preserved in the Museum (Section I, No. 5699). It is unusual to charge case shot with leaden balls; but it is not probable that the man was simultaneously wounded by a cannon-shot and musket-ball, and it is most likely that the leaden ball and the fuse-screw came from the same projectile.

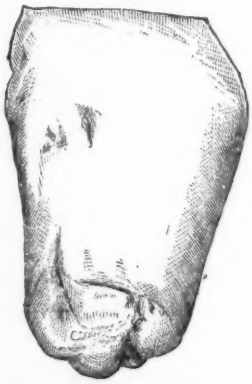
On September 1st the Satterlee Hospital report states that the stump was granulating satisfactorily, and that the patient, though weak and anæmic, and suffering from a small bed-sore over the sacrum, was yet progressing comparatively well. He continued to improve slowly during October; but in November the condyles of the femur were so much exposed, it was thought best to resect the protruding femur, which was done on December 2d. For the next two months the patient, transferred to another ward in Satterlee Hospital, had an ill-conditioned suppurating stump, with necrosis of the extremity of the femur, and suffered such

[1] Circular No. 7, War Department, Surgeon-General's Office, Washington, July 1st, 1867. A Report on Amputations at the Hip-joint in Military Surgery.

[2] Op. cit., pp. 53, 87.

pain as large doses of the salts of morphia failed to alleviate. On February 4, 1865, an incision was made through the skin and fascia-lata on the outside of the thigh, and an abscess was evacuated, together with a bit of woollen cloth. After this there was less burrowing of pus, and the general condition of the patient amended, and on May 25, 1865, he was sent to his regimental headquarters at Fort Washington, Maryland. Abscesses continued to form within the stump, which Acting Assistant Surgeon J. H. Baynas endeavoured to drain by two setons or drainage tubes. The suppuration became gradually less, and the stump assumed the appearance indicated in the accompanying wood-cut (Fig. 1), which is copied from a plaster cast. The patient was furnished, in October, with an artificial limb of the Jewett pattern; but could not use it with comfort. The stump continued irritable and tender through the winter, and on January 12, 1866, Fabry was discharged from service on surgeon's certificate of disability, and the headquarters of his regiment being removed from Fort Washington, he was taken to the Soldiers' Home on January 31, 1866. The record of the hospital at Soldiers' Home states that the pensioner was brought there on an ambulance, his stump much inflamed, discharging a large quantity of pus, that there was exfoliation of the end of the femur, and sleeplessness from excessive local pain, and protracted use of anodynes. During the spring and summer of 1866 the patient suffered from the usual epi-phenomena of chronic osteomyelitis. On October 27, 1866, Assistant Surgeon J. S. Billings, U. S. A., in consultation with Surgeon C. H. Laub, U. S. A., examined the patient, and found three fistulous openings on the external aspect of the thigh, one three and a half inches below the trochanter, another midway, and a third at the end of the stump. All of the tissues near the femur were found to be bound up in an indurated mass, "which rendered anything more than a linear exposure of the bone very difficult." It was evident that there was a thick involucrum, with small cloacæ, inclosing a large cylindrical sequestrum. An incision having been made, Dr. Billings enlarged one of the openings in the involucrum with a trephine, and removed some portions of dead bone; but the main sequestrum was not detached at that time, and Dr. Billings remarked that it was even then evident that a re-amputation was requisite to eradicate the disease, as necrosis had extended quite to the trochanter major. This operation was simply designed as exploratory and palliative. Fabry was pensioned at fifteen dollars per month, and remained in the hospital at Soldiers' Home for the next three years, suffering acutely at times, from the detachment of exfoliations and the consequent formation of abscesses, and again enjoying periods of comparative comfort. His general health did not give way under this protracted series of suppurations; there was no albuminuria, and the thoracic and abdominal viscera generally seemed healthy, the nervous system alone being somewhat disordered by the

Fig. 1.



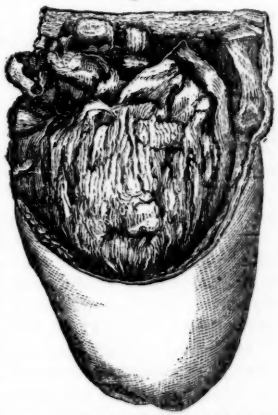
Thigh-stump, from a case of chronic osteomyelitis.

inordinate doses of anodynes in which the patient indulged. He was repeatedly advised by Surgeon Laub to undergo a re-amputation at the hip, and on several occasions, when suffering from the acute pain of abscesses, he agreed to undergo the operation; but his resolution failed when the pain was alleviated. Finally he made up his mind that he could obtain no radical relief except by a disarticulation of the limb.

On Sunday, May 15th, 1870, Assistant Surgeon George A. Otis, U. S. A., by Surgeon Laub's direction, re-amputated the limb. He was assisted by Surgeon B. Norris, U. S. A., Assistant Surgeons J. J. Woodward and J. S. Billings, U. S. A.; Drs. N. S. Lincoln, S. J. Radcliffe, J. D. Barnes, J. F. Hartigan, R. E. Williams, J. C. McConnell, C. C. Fenwick, and C. F. Nalley.

It was decided that as the mass of involucrum anteriorly, approximated so nearly to the groin, it was impossible to operate by transfixion, and that the anterior flap must be cut from without inwards. The patient being laid upon a high, narrow table, was placed under the influence of chloroform by Dr. Barnes; an aortic compressor was applied, and managed by Dr. Williams. Dr. Billings took charge of the artery at the crural arch, and Drs. Norris and Woodward were prepared to secure the larger vessels, while the remaining assistants were in readiness to perform their respective duties of abducting the stump, handing the instruments, and suppressing hemorrhage, as occasion might arise. Dr. Otis, standing on the outside of the limb, then made a long semilunar incision from a point on the tuberosity of the ischium, to a point midway between the anterior superior spinous process of the ilium and the trochanter major, dividing the skin and subcutaneous tissue, and rapidly dissected and reflected the flap within a hand's breadth of Poupart's ligament. Then with a single sweep of the knife, the muscles of the inner and interior part of the thigh were divided, and the joint laid open. Dr. Billings, following the incision with his fingers, grasped the femoral vessels in the flap, and they were promptly secured by Drs. Norris and Woodward. The femoral had been divided below the bifurcation; and it, and the profunda as well, lay in an inodular tissue of lardaceous consistence. The coats of these vessels were so soft that three successive ligatures were placed on each of them. The first two cutting through their coats. This caused a delay of nearly five minutes. The operation was then completed by connecting the angles of the first incision by a horizontal cut through the gluteals, and the division of the remaining articular attachments. From the posterior incision there was little or no bleeding. The femoral, profunda, circumflex, and obturator arteries were the only ones tied. The hemorrhage was entirely venous, the assistants performing their duties so faithfully that not a single arterial jet was seen. The gluteals and other larger trunks were faithfully

Fig. 2.



Specimen showing the line of incision in a re-amputation at the left hip. Spec. 5685, Sect. 1, A. M. M.

looked for where the normal anatomical distribution would have found them, but unavailingly. The anxiety of the operator, and of some of the older surgeons present in regard to the security of the vessels in the anterior flap was so great, as the vessels had given way under large ligatures carefully applied, that it was determined, on the suggestion of Dr. Norris, to pass directly through the flap on either side of the femoral, a little below the crural arch, a needle armed with a rather large silver wire, which was twisted over a cylinder of roller bandage, and thus made to control the vessel beyond peradventure. The patient promptly reacted, and was placed in bed, surrounded by warm blankets, and took a glass of brandy and water, and a plate of soup with relish. He had a good deal of pain in the stump at the point where the crural nerve was pinched, but otherwise was very comfortable during the afternoon. The wound had been left open, and at the expiration of three hours, Drs. Otis and Billings, who had remained with the patient, sponged the posterior portion with a warm sponge, and again looked carefully for the branches of the posterior iliac; but the arteries, though thus solicited, did not bleed. The large semilunar flap was then replaced, and was found to coaptate perfectly. No sutures or adhesive straps were applied, and the stump was simply covered with a linen compress dipped in a lotion of one teaspoonful of carbolic acid and a quart of water. The patient took a half a grain of morphia immediately after the operation; and this dose was repeated in an hour. The patient had been for years in the habit of taking very large doses of morphia, averaging not less than three grains a day. At four o'clock, as he still continued somewhat irritable, he had another dose of a single grain of morphia. He was comparatively comfortable during the evening, and partook freely of beef-tea and dilute brandy and water. Two hospital stewards and a nurse were detailed to watch him constantly. At ten o'clock his pulse was 110, temperature in the axilla 100°, the secretions natural, countenance tranquil. The dressings were scarcely stained by oozing. He had three hours' sleep towards morning.

May 16, *noon*. Pulse 112, temperature 100°; he had passed, with some difficulty, a copious amount of clear urine. He was very cheerful and hopeful. He had taken some toast and tea and rare beefsteak for his breakfast, and subsequently beef-tea and milk-punch. He had eaten with much relish. He complained only of the local pain in the stump. 5 P. M., pulse 120, temperature 100°.5. 9 P. M., pulse rose to 132, temperature fell to 96°. After midnight his pulse continued at 132, temperature at 98°, and there were spasmodic twitchings in the stump. He took two grains of sulphate of morphia, and soon fell asleep and slept for three hours. 3 A. M., pulse 110, temperature 99°.5. 5 A. M., pulse 118, temperature 99°.5. 7 A. M., pulse 130, temperature 98. 10 A. M., pulse 130, temperature 100°.5. 11 A. M., pulse 132, temperature 101°.

18*th*. Rested well last night, having about four hours' sound sleep. He took nourishment freely—had beef-tea, eggs, strawberries, tea, and toast. At nine the stump was dressed, and it was found that the wound had united by first intention for nearly three-fourths of its extent, from the interior angle over the ischium to a point about three inches to the former position of the trochanter major, and that cicatrization had made rapid progress. The upper and outer part of the wound gaped, and was approximated by one silver suture and by broad bands of adhesive plaster. The patient was moved to a new bed, and made dry and comfortable. His bowels had not been moved for four days, and were much distended

by flatus. He had an enema of an infusion of mint, with two ounces of castor oil and a few drops of spirits of turpentine. Recovering from the effects of the chloroform which had been given to produce only partial insensibility, he expressed himself as feeling much more comfortable than before the dressing. Nutritious food, and such an amount of opium as he might crave, was the treatment directed. 9 A. M., after undergoing the first dressing, his pulse was at 112, and his temperature 99°.5. The urine of the previous night had a specific gravity of 23, and gave a sediment which looked like a lateritious deposit; but it was found to be composed of mucus chiefly. It decomposed rather rapidly, and at noon on Wednesday it was found to be nearly neutral; the clouds of mucus were abounding in vibriones. The patient was rather a scholarly man, was cheered by learning that his kidneys were sound, and showed the same cheerfulness and hopefulness that he had manifested since the operation; but was a little more nervous, and disposed to loquacity. 6 P. M. Suffering a good deal from tympanitis; has not had an alvine evacuation or voided his urine during the day. He had taken no morphia, yet had dozed two or three hours. There was less local pain, but a good deal of fretfulness and irritability. He had taken a fair quantity of beef-tea, ice-cream, and eggs. At nine he had a grain of morphia, and slept for an hour. At half-past ten he had about an ounce of brandy, and he went to sleep; at which time the temperature was 98°.5, and pulse 105.

19*th*, 2 A. M. Has slept since half-past ten P. M.; temperature 100°.5, pulse 114; says he is "flighty," but has no signs of delirium; skin moist. At four A. M., temperature 99°.5, pulse 105. At six A. M., temperature 99°, pulse 102. Half-past six A. M., took a little brandy. 8 A. M., ate some cracker and ham, and took a little tea; temperature 98°.5, pulse 112. 11 A. M., temperature 100°, pulse 120; catheter introduced and three pints of high-coloured urine drawn off; complains of pain in toes of the amputated limb. 12 M., one grain of morphia given; ate some bread and ham, and took some chicken-broth; temperature 99°, pulse 115. 1 P. M., resting well. 3 P. M., complains of pain in amputated foot; tongue furred; one grain of morphia administered; temperature 100°.5, pulse 113. 6 P. M., took a little ham, tea, and cracker; nervous twitching in the stump. Half-past seven P. M., temperature 100°.5, pulse 108. 9 P. M., fretful; prolonged nervous twitching in stump; one grain of morphia given; slept three hours.

20*th*. At seven A. M. a purgative enema composed of an emulsion of castor oil, turpentine, common salt, and flaxseed-tea, one quart injected, without immediate result; pulse 104. He breakfasted on chicken, toast, and eggs at nine A. M.; he took two glasses of sherry wine. At ten he complained of suffering, and took a grain of sulphate of morphia. At noon a catheter was introduced and twenty-four ounces of urine were drawn off; pulse 102, temperature 100°.5. He was much depressed by the persistent constipation. A dose of Seltzer salts was given. At four he became irritable, and took a grain of morphia. In three-quarters of an hour this dose was repeated. The stump was then dressed, and the acupressure wire from the femoral artery was removed. The flap had united in nearly its whole extent, and the granulating surface presented a very healthy appearance. The suppuration was very slight. At seven in the evening, pulse 112, temperature 100°.2. At eleven he took one grain of morphia, having slept restlessly since nine. He passed a good night, pulse averaging 96, and temperature 98°.5.

21*st*. The tongue, for the first time, was a little furred; pulse 96, temperature 98°.8. Breakfasted at nine on toast and tea, rare beefsteak, and sherry wine. Noon, pulse 96, and temperature 100°; complained much of pain, and took a grain of morphia. At three, pulse 108, temperature 100°; suffered pain in bowels, and from repletion of bladder, till his urine was evacuated by a catheter. At seven, had a very free evacuation of the bowels; pulse 108, temperature 98°.2. At nine, pulse 102, temperature 100°. He slept soundly from ten at night till four in the morning; awoke with a good pulse (88), and temperature 100°.

22*d*. Said he felt a great deal better, and no pain in stump; pulse 108, temperature 99°. At six A. M., the bowels were freely evacuated. At eight, pulse 108, temperature 100°.2; patient was quiet and cheerful, whistling occasionally; urine evacuated by catheter; eat for breakfast toast and eggs, a bit of rare steak, bread, and a julep made with sherry wine, and little lettuce salad, which he craved. At ten, pulse 96, temperature 100°; dressings were removed and replaced without much suffering, and the patient transferred to another bed. At noon, after the change of bed, pulse 96, temperature 101°.2. At two P. M., pulse 108, temperature 102°. At four, pulse 92, temperature 101°. At six P. M., pulse 108, temperature 100°.8. 7 P. M., 1 grain sulph. morphia; tongue had cleaned off. 9 P. M., pulse 110, temperature 101°. 10 P. M., urine drawn off by catheter, and 1 grain sulph. morphia given. At midnight, pulse 106, temperature 100°.5.

23*d*. 3 A. M., pulse 100, temperature 98°.5.; slept restlessly at short intervals throughout the night; he seemed very bright in the morning; pulse 90, temperature 96°; he soon, however, became low spirited, perspired freely, and complained of great weakness. 10 A. M., pulse 94, temperature 98°; still perspiring freely; had eaten a hearty breakfast of chicken, poached eggs, and toast; catheter introduced. At three P. M., pulse 100, temperature 98°. At half-past six P. M., pulse 104, temperature 100°, complained of pain, and 1 grain morphia given. At nine P. M., pulse 100, temperature 99°, took a grain and a half of morphia. At two A. M., pulse 100, temperature 99°.5, took another grain and a half of morphia.

24*th*. At half-past five P. M., pulse 95, temperature 98°, had slept absolutely none during the night. At nine A. M., pulse 100, temperature 99°. At twelve, pulse 90, temperature 98°, patient much more cheerful; breakfasted heartily on toast, poached eggs, chops; urine drawn off by a catheter; took beef-tea freely during the day, and sherry wine; he dined with relish on beefsteak, and vegetables and strawberries; had a tolerably comfortable day; slept none last night; urine drawn off by a catheter in the morning—rather high-coloured, and turbid, with phosphates. At six P. M., his stump was dressed by Dr. Otis. The granulations were everywhere healthy, and suppuration very slight, and small abscess formed in the track of the acupressure wire. A minute incision was made, and about a drachm of thick creamy pus was evacuated; the edges of the outer part of the wound, where it gaped, were approximated by five strips of adhesive plaster; the wound was then covered by a compress dipped in carbolic acid solution; over this was placed a mass of picked oakum, and the whole was secured by a figure-of-eight roller bandage. The patient ate quite heartily during the day of mutton chop, toast, tea, and beef-essence, and drank a bottle of ale. At eight o'clock in the evening he took a grain of opium and half a grain of

camphor. At ten o'clock he was sleeping, pulse 92. At twelve o'clock he was perspiring freely; pulse 90, temperature 97°.

25*th*. 1½ A. M., he urinated one pint, and took another camphor and opium pill. From this time till 6 A. M. he slept soundly, perspiring freely. At six, pulse 91, temperature 98°. At eight he breakfasted on coffee, eggs, and toast. At ten A. M. he was cheerful and bright; pulse 96, temperature 98°. At twelve his pulse was 92, and temperature 98°.5.

It is hardly necessary to pursue further the minute reports of the hospital stewards who watched the case. These were Hospital Stewards Duncan, Williams, McConnell, Cheney, Sawtelle, Schwawecker, Brennan, and Fisher, U. S. A., all of the Surgeon General's office, whose faithful supervision greatly contributed to the patient's safety. I have thought it well to retain the minutes of the temperature, pulse, etc., for comparison with the clinical notes in similar cases.

Fabry went on to improve without any further complication of importance, and on June 5, or twenty-one days after the operation, he was able to be about the hospital on crutches; and soon afterwards to ride about the grounds at the Soldiers' Home. On July 13, 1870, Surgeon Laub, U. S. A., sent him down to the Army Medical Museum, and a photograph was made of his stump (Photograph 276, *Surgical Series*, A. M. M.). It was then completely cicatrized, though in one or two places the delicate new epidermis was slightly abraded. In November, Fabry was again at the Museum, in good health, with a sound stump. At this date, December 8, 1870, he is in good condition, working at his favourite pursuit of clock-repairing, at the Soldiers' Home.

Fig. 3.

Necrosis of a femur exarticulated six years after a previous amputation.—Spec. 5684, Sec. 1, A. M. M.

The stump was dissected at the Army Medical Museum, and it was found that the entire shaft of the femur was necrosed and enveloped in an involucrum of masses of foliaceous callus of extraordinary magnitude. A posterior view of the specimen is given in the accompanying wood-cut.

Amputation at the hip, after previous amputation in the continuity of the thigh, was first performed by Guthrie in 1812, and has since been repeated twenty times at least. Sir Astley Cooper operated in 1824 on a man of forty, with cancer; the patient survived at least two years. In 1835 Mayo successfully exarticulated at the hip, in the case of a woman of twenty-two years, four years after amputation at the knee. Cajetan Textor in 1841 performed an intermediary operation, with success, on a man who had gangrene of the stump after amputation at the knee. In 1844 Dr. Sands Cox successfully reamputated at the hip, in the case of a woman of twenty-five, who had undergone an operation during childhood for disease of the left knee-joint. In 1848 Mr. Syme repeated the operation in a case of necrosis following an amputation in the thigh, performed

three months previously, for compound fracture. This was the first successful hip-joint amputation in Scotland. In 1850 Dr. Van Buren operated successfully on a man of forty-three years, whose thigh had been amputated two years before for osteochondroma. This was probably the first hip-joint amputation performed under the influence of chloroform. Dr. Bradbury successfully reamputated at the hip in 1851 in the case of a scrofulous boy, six months after an amputation in the lower third for disease of the knee-joint. Dr. Fayrer in 1853 performed an intermediary reamputation at the hip on a man of thirty-six years; the case terminated fatally in five days. In 1860 Mr. Hancock reamputated at the hip on a man of thirty-five, who, on account of suppuration in the knee-joint, had been amputated in the lower third of the thigh in December, 1858, and again, in August, 1859, at mid-thigh, for ostitis. The patient survived the reamputation twelve hours. Mr. Fayrer again reamputated at the hip on April 24, 1864, in the case of a boy of sixteen years, whose knee-joint had been injured by a fall from a horse, and who had been amputated at the lower third twelve days previously. This operation was successful, and a wood-cut representing the appearance of the stump sixteen months after the operation is given at page 629 of Mr. Fayrer's *Clinical Surgery in India*. In 1864 there were three other examples of reamputation at the hip after previous amputations in the continuity of the thigh. Dr. Mott in May operated successfully on a soldier who had been amputated at the lower third of the thigh on account of a bayonet wound of the right knee at the first battle of Bull Run; his patient is still living. Dr. Hassenburg operated in September on a soldier aged twenty-three years, shot through the left knee, and amputated at mid-thigh two months previously; but the patient died of pyæmia a week after the operation. Dr. Gurdon Buck also operated in September, 1864, in the case of Lieutenant Hawkins, aged twenty-three years, who suffered a fracture of the lower third of the right femur in June, 1862, and had been amputated at mid-thigh in April, 1863. This operation was unsuccessful, the patient dying in a syncope the following day; yet the undertaking of the operation was, I think, as noble an action as the leading of a "forlorn hope." In 1865 Drs. J. H. Packard and A. M. Fauntleroy successfully reamputated at the hip. In both instances the operation was performed for chronic osteomyelitis following amputations of the thigh for gunshot injury. In 1866 Drs. T. G. Morton and J. B. Whitcomb reamputated at the hip for chronic osteomyelitis following amputations in the continuity of the femur for gunshot injury. Dr. Whitcomb's patient, twenty-three years of age, was in a very depressed condition, and survived the operation but a few hours; but Dr. Morton's patient, aged twenty-six years, is now in excellent health. In 1867 Mr. Fayrer performed his third reamputation at the hip in the case of a Hindoo lad of twenty-one years. It was an intermediary operation, performed a few days after an amputation of the

thigh for an incised wound of the knee, and terminated fatally. Mr. Le Gros Clark last year successfully reamputated at the hip in the case of a woman of twenty-one years, who had undergone an amputation low down in the thigh for malignant disease of the knee.

I have not had leisure to make an exhaustive search of the journals, and may have omitted some cases of reamputation at the hip that have been recorded. The twenty-one cases enumerated, if not all, are probably nearly all that have been reported. Nine of them belong to military and twelve to civil surgery. But this distinction is valueless, as the causes requiring the operation are strictly analogous in both branches of practice. It is more important to observe that the operation was performed in twelve cases for that affection of the shaft of long bones after amputations which we sometimes call "chronic osteomyelitis," and still imperfectly understand; and that six were performed on account of the recurrence of malignant disease, or neuroma, or scrofulous degeneration, while three were compulsory operations for hemorrhage or gangrene. Fourteen of the twenty-one cases, or sixty-six per cent., were successful. We may conclude, therefore, that in incurable disorders of thigh stumps, resulting either from injury or disease, disarticulation at the hip is the proper remedy.

In necrosis following amputations of the thigh, I have known of but one instance of the extension of the disease to the neck of the femur, viz., Dr. Morton's case, reported and figured at page 20, vol. lii. of this Journal; but when the entire diaphysis is involved in disease, I believe disarticulation will give better results than amputation through the trochanters. While in primary and intermediary amputations we sacrifice nothing that may be saved from the knife, in the secondary or remote operations it may often be advisable to resort to disarticulation. And if this is true in regard to secondary amputations performed for incurable diseases of the largest and longest bone in the body, must we not believe, also, that in incurable chronic osteomyelitis, or malignant disease of the tibia, humerus, and bones of the forearm, disarticulation at the proximal articulation is the appropriate treatment?

I add a paragraph to refer to a prothetic apparatus for the survivors of amputation at the hip. By using a gutta-percha artificial stump nine or ten inches in length, firmly strapped to the pelvis by a broad chamois-lined canvas band, an ordinary artificial limb for thigh-stumps can be used. In two cases under my supervision this expedient has been employed by Dr. E. D. Hudson, 696 Broadway, New York, and the mutilated men have walked better than most men with artificial limbs who had been amputated at mid-thigh.

ART. XV.—*Acute Atrophy of the Liver.* By ISAAC G. PORTER, M. D., of New London, Conn.

THAT cases of acute atrophy of the liver are rare, in this country, may be safely inferred from the fact that in an interesting article on this subject, by Dr. John Homans, of Boston, published in this Journal for July, 1868, and consisting of an analysis of twenty cases collected from all sources, and supplementing thirty-one analyzed by Frerichs, only five of them are American. But the disease possesses a practical interest and importance out of proportion to its rarity. The means of its diagnosis in its early stages are so few and limited, that an ordinary case of jaundice, or one slightly more obstinate and deeply marked in its symptoms than usual may suddenly issue in a disastrous and fatal termination. Those who are familiar with *Budd on the Liver* will recollect the section entitled "Fatal Jaundice," under which name he describes, substantially, this affection; a nomenclature which, if less accurate and scientific, yet brings a leading symptom of the early stages prominently before the mind, and affords the lesson that jaundice, though usually amenable to treatment, may suddenly prove fatal. The term, it is true, may perhaps with equal propriety be applied to cancerous and other affections of the liver, and it is well known that icterode symptoms often show themselves in pyæmia, yellow fever, and some inflammations which are usually mortal. These, however, have their diagnostic phenomena by which they may be known, but with the exception of increased resonance of the right hypochondrium arising from the comparative absence of the liver which exists, there is no prominent or distinctive sign of the disease in question, prior to the supervention of cerebral symptoms.

The following case, which, it is conceived, is typical of this affection, deserves a record as descriptive of the general features usually existing, and confirmatory, mainly, of such as are described by Bright, Alison, Graves, and emphatically, Frerichs.

A merchant, æt. 23 years, visited Savannah, Georgia, in the spring of 1870, spending the month of April. Malarial influences were then and there prevalent, resulting in a mild remittent, and after his return, a smart intermittent, which yielded to ordinary treatment, but left him in a measure sallow and debilitated, yet not sufficiently so to preclude his ordinary vocations, which kept him much in the hot sun.

July 7. Applied for advice on account of gastric derangement, which in a few days was followed by icterode symptoms, he continuing to call on me as late as the 18th inst., and in the mean time attending to light business. He now became languid and indisposed to exertion; pulse 50; skin quite icterode; slight tenderness over the region of the stomach and duodenum, but none at any time over the liver; urine abundant and loaded with the colouring matter of the bile; feces clay-coloured; the case presenting the ordinary signs of gastro-duodenitis, to which the jaundice was

referred. Nausea was almost constant, with occasional vomiting; bowels constipated. Epistaxis was a troublesome symptom in the last week, but being in a measure habitual, it at first gave rise to no apprehension.

The case was treated with laxatives, and one or two active mercurial purgatives, alkalies, and warm baths. On account of nausea and an uncomfortable feeling in the epigastrium, a blister was applied, and subsequently, eight leeches, both temporarily relieving the disagreeable symptoms. Ox-gall in pills was given with reference to constipation, and after a free use of it, the only alvine discharge which was not clay-coloured was obtained. But the darker hue may have resulted from an admixture of blood. The appetite was poor throughout, and for two days before death was entirely gone. Such aperients as the combination of sulphate and carbonate, of magnesia, so highly praised by Budd in cases of suppression of the secretion of bile, and Congress water, had no effect on the bowels, and there were increasing weakness and sallowness, the latter becoming intense and of a dark hue.

29*th*. Until this day no cerebral symptoms have presented themselves. Now, a peculiar *light* feeling, or swimming of the head showed itself, followed by delirium, mild at first, but soon becoming noisy and violent, attended by moans, jactitation, and profuse epistaxis. The leech-bites also bled, abraded as they were by his rolling from side to side, and so difficult was it to restrain him, that twelve drops of Magend. solution of morphia were given, hypodermically, with entire success for a period of eight hours. Still sleep was not profound, and he continued to lose blood from the nose. His hands were tremulous when extended; alternate flushing and paleness coursed over the countenance, and sensation even now was so obtuse, that fluids placed in the mouth were unnoticed.

30*th*. The patient was visited by Prof. Austin Flint, Sen., of New York; but the disease was in its nature so violent and had made such progress that there was little room for medication. The case was regarded as fatal. The urine, which had all along been abundant, became sparing after delirium set in, and the cerebral symptoms were referable, as he thought, to uræmia. The patient was able to take little besides iced-champagne and iced-milk.

In the afternoon there was a recurrence of the violent delirium. Hæmatemesis accompanied the epistaxis, the blood vomited being black and containing flakes like coffee grounds. To quiet the patient, ten grains of chloral. hydrat. were given, and in six hours repeated, with good results. From this time to his death, which occurred July 31st, at 10 A. M., his pulse, which at the outset was 50 per minute, and subsequently for ten days nearly natural, now became more and more rapid, ranging from 100 to 140, and until one hour before death it continued full, hard, and bounding, with a great increase of heat. At death the thermometer in the axilla stood at 103°, the pulse for the last hour becoming weaker, until it ceased.

Post-mortem examination thirty hours after death, conducted by A. W. Nelson, M. D., in the presence of most of the physicians of this place.

The surface of the body was deeply jaundiced, the posterior portions being of a purple hue, probably from hypostasis of blood.

The stomach and duodenum exhibited no marks of inflammation, the former containing much grumous blood and mucus, similar to that which had been vomited during the last day of life. The gall-bladder was entirely empty, and the bile ducts open. The liver was extremely small, amounting to scarcely half its usual size, and was of a light yellowish-brown colour, shrivelled on its surface, and less firm than ordinary, yet not

so disorganized as to render it "capable of being torn by the fingers," as is related of its condition in some analogous cases. The kidneys were softened, and of a dark, congested aspect.

The liver was examined microscopically and showed entire absence of the cell-element. I regret to say that it was not weighed. It was so small, however, that on opening the cavity of the abdomen, the universal inquiry among by-standers was, "Where is the liver?" since it had shrunk so much as to have nearly vacated its normal position.

The uriniferous tubes of the kidneys were found devoid of cells, and filled with granular matter.

The following is an analysis of the urine: Reaction acid; specific gravity 1015½; bile pigment present in very large quantity; no albumen.

In the deposit was found a large number of cells, probably from the kidneys, and casts of various sizes, granular and epithelial, tinged with the colouring matter of the bile. The urine was not evaporated for the purpose of ascertaining the existence, or non-existence of crystals of leucine and tyrosine.

A single remark is added respecting the etiology of this disease. Aitken says "the circumstances under which the affection has occurred are the following: mental distress, dissolute habits, venereal excesses, drunkenness, nervous depression, and the influence of miasmata," and others entertain similar opinions. If either of these causes were in operation in the present case, it must have been the latter alone, viz., miasmata. While he had the anxieties incident to a large commercial business at an early period of life, he was a man of excellent habits, and living under happy mental and hygienic influences.

TRANSACTIONS OF SOCIETIES.

ART. XVI.—*Summary of the Proceedings of the Pathological Society of Philadelphia.*

1870. *June* 9. ***Phthisis diagnosticated by aid of the Microscopic Examination of the Sputa.***

DR. HUTCHINSON exhibited the specimens, and said they were taken from the body of a young man, aged 19 years, who was admitted into the Pennsylvania Hospital February 17th, 1870. The lad was a sailor, said to have had no hereditary predisposition to phthisis, and to have been suffering for four weeks with his present illness, which had begun as an ordinary cold, but was now thought to be a fever. On admission, there was marked mental hebetude, decided heat of skin, increased frequency of pulse and of respiration, and occasional cough with scanty expectoration, but no diarrhœa, no tympany, no rose-coloured spots or other eruption, no epistaxis, and only a slight coating of the tongue. Careful physical examinations made at various times and by different physicians did not lead to the detection of tuberculous disease of the lungs, although this was suspected. Thermometric observations made daily for five weeks showed that the temperature of the body was abnormally high, as it was never found to be below 101°, and was frequently 104°. On a few consecutive days the thermometer would indicate the latter temperature, but this was by no means the rule, and there was at no time anything like a gradual rise or fall of temperature noted.

Failing to detect by means of physical examination disease of the lungs, to which this long-continued elevation of the temperature, as well as some of the general symptoms pointed, I requested Dr. J. G. Richardson, microscopist to the hospital, to examine the sputa of the patient, after they had been treated as recommended by Fenwick, by adding a solution of caustic soda and boiling, and on the 14th of March I received a reply to the effect that the very scanty expectoration contained a very large quantity of yellow elastic tissue. A week later dulness on percussion and some harshness of the respiratory murmur was detected in the left superior scapular region, and these signs soon gave place, not merely in this region, but in others, to those of softening. The thermometric observations taken from time to time, always indicated an elevation of temperature. In April there were symptoms of tubercular meningitis, but these afterwards disappeared, and did not again recur. On the 8th of April the patient complained of intense pain in his abdomen, which was somewhat increased by pressure. This pain caused the patient to draw up his lower extremities, and to bend forward his body so as to

relax the abdominal walls—a position which suggested peritonitis, but there was no other evidence of its existence. The pain continued until the time of his death, but was always moderated by the administration of opium.

Death took place June 4th. The autopsy was made the next day, 30 hours after death.

There was a good deal of serous effusion beneath the membranes and in the ventricles of the brain, but no evidence of tubercular or other inflammation of the membranes or of the brain itself. The bronchial glands were enormously enlarged. At the apices of both lungs were numerous small excavations; the remainder of both lungs was studded with tubercles. The mesenteric glands were also much enlarged, and the intestines were the seat of tuberculous ulceration.

Cystic Sarcoma of Breast.—Dr. S. ASHHURST presented the specimen and history.

Miss —— aged 63, single, noticed about May 1, 1870, a slight lump in the right breast, accompanied with a little discharge from the nipple. She applied some domestic remedies until June 2d, when she sought medical advice. There was then retraction of the nipple and a very prominent tumour above it, in which existed marked fluctuation. An exploratory puncture was made, which gave exit to a little honey-like fluid and blood. A similar discharge issued from the nipple. Dr. J. Cheston Morris was called in, with whom I saw the case in consultation, on June 4th. The general condition was one of exceptionably perfect health. There was very slight pricking pain in the breast. The skin was much congested. There was very slight discharge from the nipple and the puncture of the 2d instant. There was no appreciable enlargement of the axillary or supra-clavicular glands. There were no large veins on the surface of the breast. The tumour was freely movable under the skin and upon the pectoral muscle, of firm but not stony consistence. The growth was removed by Dr. Morris upon the 7th instant. The tumour was referred to the Committee on Morbid Growths.

Report of Committee.—The tumour had been soaked in spirit. The specimen presented marked retraction of the nipple and discoloration of the skin over the most prominent part. The tumour was about $1\frac{1}{2}$ inch in diameter. Its periphery was hard and fibroid, while the central parts were soft, friable, and, in some places, even pultaceous. All parts of the mass gave exit to a milky fluid on pressure.

The cells in the softer parts of the mass were large, rounded, with markedly granular contents, with one, or in rare cases, two nuclei, which were large in comparison with the containing cells. There was no longer any trace of cystic disease.

The tumour would then appear to be a cystic sarcoma.

Extensive Ulcerative Disease of the Large Intestine.—Dr. TYSON exhibited the entire large intestine with a portion of the small bowel, and remarked as follows:—

R. T., aged 28, was quite well previous to March 1st, 1869, when, after exposure to cold, was seized with what appeared to be true dysentery, since the attack was characterized by stools of blood and mucus attended by much tenesmus. This continued more or less interruptedly until about

June 1st following, when he again considered himself well, and was attending to business. Early in June, however, he ate freely of strawberries, and immediately the symptoms returned. He seems to have been treated by opiates and astringents, with varying results; at times he was comparatively comfortable, but the discharge of blood and mucus continued more or less uninterruptedly. In November, his medical adviser instituted treatment by cauterization of the rectum, with apparent good results. He continued, however, losing strength, and finally gave up all attention to business. Towards the close of 1869, he was for a short time under "Thomsonian" practice, but from January 1st to May 23d was under the care of a homœopath, during which time he grew gradually worse.

May 24, when I assumed charge, he presented the following condition: Much emaciated, skin dry and harsh, pulse 107; more or less constantly a mixture of blood and mucus passes from rectum without any effort of patient. In addition, there are from six to twelve evacuations in twenty-four hours, of the same elements, occasionally mingled with fecal matter. Marked œdema of feet, legs, and thighs exists; also ascites, abdomen measuring 32½ inches at umbilicus. In recumbent position, dorsal decubitus, the right lung is markedly resonant anteriorly as far as sixth rib, where is met the upper edge of the liver, somewhat pushed up by abdominal effusion; resonance in right axilla to same point. On left side anteriorly, resonance to fifth rib, where dulness of heart commences, soon to be replaced by downward gastric tympany. In left axilla, dulness extends nearly to lower border of ribs, where tympany of distended and displaced stomach begins. On sitting up, physical signs on right side remain unaltered. On left side line of dulness in axilla descends to eighth rib, while vocal fremitus is also diminished in this situation—phenomena no doubt due to liquid in left pleural cavity.

The patient responded but feebly to general restorative treatment, and local astringent (sulphate of copper) solutions to rectum, for twelve days, but on the thirteenth day after coming under my notice, he began rapidly to sink, and died in about twelve hours less than two weeks after I first saw him.

Post-mortem four hours after death.—No peculiarities of surface. At least four quarts of effused serum were taken from abdominal cavity, about two quarts from left pleural cavity, and a very small quantity, an ounce or two, from the right. The viscera were all markedly blanched and transparent. The entire large intestine was removed, and found to be in the condition presented by the specimen, in a state of continuous ulceration from the sphincter muscle to the ileo-cœcal valve (the ulcers being everywhere deep and excavating, but in no instance perforating). As is also noticed, the Peyerean patches and solitary glands to the distance of twelve inches above the valve are also ulcerated, but beyond this the bowel is healthy.

The presence of the bluish stain indicates how short a distance the injections administered in the ordinary way ascended the bowel, not more than four inches above the sphincter muscle; proving also the absolute necessity of using some means other than those usually employed, of throwing solutions into the lower bowel, if it be desired to reach a point more than four inches from the anus. In this case the inroads of the disease had been so long unresisted, and condition of the patient so utterly beyond hope, that little was expected from general or local treatment.

Encephaloid of the Testicle.—Dr. S. W. GROSS exhibited an encephaloid cancer of the left testicle, removed on the 26th of May from a strong, muscular gentleman, thirty-two years of age, in whom there was no history of cancerous or syphilitic taint. He had noticed from his earliest youth that the gland was larger and harder than its fellow, and at the age of eighteen years—the testicle gradually increasing in the interval—he began to wear a suspensory bandage, on account of severe, intermittent pain in the cord. In April, 1869, he received an accidental blow on the part, and in the following October, when the organ was about one-third the size of the mass at the time of its extirpation, it commenced to grow rapidly without any signs of inflammation. Within the past few months, it had been the seat of great pain on pressure at a spot at its upper and anterior limits. From the outset the organ had been equably enlarged, without any unevenness of surface. It had a uniform, full, firm, and dense feel, without elasticity or apparent fluctuation at any point. The integuments of the scrotum were sound and unadherent, and there was no enlargement of the superficial veins. The cord was large from a varicose state of its vessels, but appeared healthy in other respects. There was no evident enlargement of the inguinal or lumbar lymphatic glands, nor was there any fluid in the sac of the tunica vaginalis. After removal, the tumour was found to be oval, somewhat flattened from side to side, eight inches and a half in its transverse circumference, by ten inches and a half in its vertical, while its weight was ten ounces. The veins of the cord were varicose and enlarged, but the cord was otherwise healthy. On section, the mass resembled hardened brain-tissue, with, here and there, points of linear injection. By scraping, a creamy juice exuded, which showed, microscopically, innumerable large free nuclei and granular cells, a few of which possessed dot-like nuclei.

Dr. C. B. NANCREDE presented, for Dr. A. G. HEYL, a specimen of *tumour within the spinal meninges*, with the following history:—

Wm. F——, æt. 20, unmarried, blacksmith's apprentice, admitted May 16, 1870, to the male medical ward of the Episcopal Hospital. Three weeks previous he had received a severe wetting during a heavy storm, and neglected to change his garments. One week subsequent to this, while in the act of lifting some heavy cart-wheels, he was seized with a violent pain in the lumbar region, which was, apparently, the commencement of his illness. The first week after this occurrence was marked by slight difficulty of micturition and locomotion. During the second week complete retention of urine obtained, and also paralysis of motion and sensation.

On admission there was complete inability to use the muscles of the lower extremities; skin hot; pulse frequent; tongue dry and red; anorexia; no movement of bowels for a week. Mind perfectly clear; special senses intact. Urine, when drawn off by catheter, was turbid, showing, on microscopical examination, an abundance of pus. A large sloughing bedsore occupied the upper portions of both buttocks, and also marked œdema on the dorsal aspects of both feet. All the muscles of the lower extremities responded feebly to electric current. Hyperæsthesia over surface of the trunk. Temperature high throughout, reaching its maximum, 105°, on the fifth day after admission, ranging from this to 101°. Pulse frequent, rather strong, ranging from 112 to 124. No spasms or convulsions, although reflex contractions could be readily excited. Towards the close of the disease, muscular tremour was observed.

Three days preceding death slight hebetude was observed, with a tendency on the following day to delirium, which was followed by subsultus tendinum, and rapid, laboured breathing. Coma supervened twenty-four hours before the fatal termination.

Post-mortem examination, twelve hours after death, revealed tuberculous deposit in the lungs, principally at the apices of the lungs, with a small cavity in one; also hypostatic congestion; liver healthy; spleen much enlarged; kidneys slightly congested; bladder normal. On removing the calvarium, the meninges showed marked congestion; but neither serum nor lymph. Slight congestion of brain-substance; no serum in ventricles. Spinal meninges much congested; opposite the lowest dorsal and uppermost lumbar vertebræ an oblong white mass was found, about an inch in length.

Referred to Committee on Morbid Growths, which reported as follows:—

The spinal cord had been hardened in alcohol. Sections of the cord were made at short intervals throughout its length. Down to the middle of the dorsal region the structure of the cord was apparently entirely healthy, and symmetrical in all its parts. Below this point there was apparently a slight difference in the size of the posterior horns of gray matter, in favour of the right side. No positive difference between the corresponding portions of the white matter of the cord could be detected. The consistence of the cord varied somewhat in different portions. Excepting at the point of the morbid growth, it was quite firm; at this latter part, however, the consistence was diminished.

Microscopic examination showed a healthy condition of the nerve-tissue, excepting for a short distance corresponding to the morbid growth, where it presented abundant granular fat and some granule-cells.

The morbid growth was found to lie exterior to the dura mater; it was closely apposed to this membrane, and moderately adherent to it, though it could be completely separated by slight force. Its outer surface was closely moulded to the interior of the spinal column. The structure of the mass was densely fibroid, with but occasional patches of rounded granular nucleated cells, and therefore appeared to be either an organized blood-clot or a layer of organized lymph.

Sept. 8. *Tumour of the Right Ovary.*—Dr. J. E. MEARS presented the specimen, and gave the following history of the case occurring in the practice of Dr. W. L. Atlee:—

L. J., æt. 25, unmarried, was awakened during the night of March 26, 1870, by a severe cramp pain in the right iliac region, which continued and prevented her from engaging in her usual duties on the next day. Her physician treated the attack as one of congestion of the liver. After four weeks' confinement to bed, she was able to go out, but found on dressing herself that her clothes would not meet by two and a half inches. During eleven weeks following, she increased in girth twelve inches. Becoming dissatisfied, she consulted a female physician, who treated her for enlargement of the liver and spleen. Dr. Littell was subsequently called to take charge of the case, and making a correct diagnosis advised the patient to consult Dr. W. L. Atlee.

At that time her condition was as follows: Abdomen enlarged to a size greater than that of a woman at full period of gestation—quite uni-

form, and generally hard and unyielding; at different points elastic portions are distinguished resembling superficial small flat cysts; no fluctuation; pelvis free; hymen intact; os uteri central, and uterus movable; the sound enters 2½ inches, and ascends back of the tumour. Measurement round waist 32 inches; round umbilicus 42; from sternum to umbilicus 14; from sternum to pubes 20; from right superior spinous process to linea alba 9½; between spinous processes 20 inches.

Prostration extreme; pulse very feeble and rapid; respiration short and frequent; anorexia; bowels disposed to looseness, and sleep is obtained only through the influence of anodynes.

Diagnosis.—Multilocular ovarian tumour.

July 12*th.* Paracentesis abdominis was performed, evacuating about six pints of a dark-coloured albuminous fluid; this quantity was obtained by varying the direction of the trocar, and tapping several cysts. Temporary relief was afforded by the tapping. Tongue became red; stomach rejected everything, and there was great restlessness and loss of sleep; pulse exceedingly small; skin relaxed, and respiration more oppressed. Abdomen becoming more distended and very tense, tapping was again performed August 13th, removing only two and a half pints of purulent-looking fluid. Symptoms of septicæmia continuing, the patient died August 22d, 1870.

August 23*d. Sectio-cadaveris* performed by Dr. Mears; Drs. W. L. Atlee, W. Lemuel Atlee, and John P. Bethell being present. Body extremely emaciated. Lower extremities œdematous. On opening the abdominal cavity the tumour was found firmly adherent to the parietes; surface of tumour occupied by exogenous cysts, the walls of which were quite fragile, breaking down in the attempt to separate the adhesions. These cysts contained a thick, dark-grayish fluid, with free pulpy masses. The tumour consisted of the right ovary, and was multilocular; uterus normal, and with the left ovary was removed, attached to the tumour; left ovary somewhat enlarged and congested; liver and spleen normal. Other organs not examined. Referred to Committee on Morbid Growths, which reported as follows:—

Report of Committee on Morbid Growths.—Mass presented no true stroma, being composed exclusively of cells, of very varying size and shape. Many of them resembled pavement epithelium; others columnar epithelium, while there were in addition a large variety of cells of oval, spherical, or spindle-shape. For the most part these cells contained a single large nucleus. Your committee regard this as a malignant intra-cystic growth of the ovary, of the nature of encephaloid carcinoma. On section the mass yielded quite abundant milky fluid, exuding on pressure from a lax, vascular stroma. This fluid abounded in cells, mostly rounded, and of size of gland elements, though there were others much larger. Many of these cells were advanced in fatty degeneration. For the most part, the cells contained a single nucleus.

October 14. *Fibrous Tumours.*—Dr. C. H. BURNETT presented *two* of these, removed by him from the ears of a negress, aged 28. They were situated on the lobes of the ears, and were eight years in attaining their growth. Their size was not symmetrical, the one on the right being twice as large as that on the left ear. The history showed that at the age of nineteen the woman had repeatedly pierced the lobes for the pur-

pose of wearing ear-rings, and had kept the holes in a constant state of irritation by inserting into them broom fibres, straws, etc., and although the rings were of gold and had never cut through, the growths had speedily followed the application of the above-mentioned irritants. At the time of the removal of the tumours their conjoined weight was 260 grains Troy.

Their minute structure was that of exceedingly dense white fibrous tissue, with some fibres of elastic tissue. No history of any morbid growth whatever in the family could be elicited. In fact this case seems to confirm the statement made by Tröltsch in his work on the ear, that these tumours are due to the improper treatment of the pierced lobes, and that they are usually found in negresses, more frequently in the Antilles, where the ornament worn is large and of brass. However, such growths have been constantly observed in this city, and their nature proven to be recurrent but not malignant.

Dr. W. PEPPER referred to the tendency exhibited by these tumours to recur, often twice and three times, presenting each time the same dense fibrous structure. He did not know an instance of a recurrent tumour so entirely innocent as these. He had seen five or six cases, and all were in negresses.

Severe Compound Fracture of Arm from a Slight Fall; Amputation; Recovery.—Dr. PACKARD related the following case, and showed the specimen :—

George D., æt. 8, June 13, 1870, while standing on a low paling fence, slipped off; in falling his sleeve caught and jerked him backwards. He was brought to town, losing a good deal of blood. At about 3 P.M. I saw him, at the request of Dr. J. F. Meigs.

About an inch and a half of the humerus was protruding through a wound in the skin, a little to the inner side of the median line in front of the arm. The forearm and hand were cold, and there was no pulsation at the wrist. I called Dr. Agnew in further consultation, and we decided to amputate, which I did by anterior and posterior flaps, Dr. W. Sinkler giving the boy ether. The stump did perfectly well, although the ligatures were not all away until Aug. 27, *seventy-five days* after the operation.

On examination of the limb we found a transverse fracture of the shaft an inch or more above the elbow, and a fissure running down almost into the joint. The brachial artery had been torn across by the forcing forwards of the lower end of the upper fragment. It is very rare to see so severe an injury as the result of so slight a fall.

Oct. 27. *Tumour of the Head of the Pancreas.*—Dr. WM. PEPPER presented the specimen, and said :—

John Honan, æt. 45, of very intemperate habits, first noticed hemorrhage from the bowels in early part of 1870. This recurred quite frequently, and on July 22, 1870, he had profuse vomiting of blood, followed by the passage of altered blood by stool. Two weeks later he vomited blood a second time; and on August 23 there was such rapid hemorrhage into the stomach and bowels as to prove fatal in about one hour. Throughout the attack the bowels had been habitually costive, but the stools were apparently normal; appetite was fair, and no vomiting occurred except

at the times above mentioned. The area of hepatic dulness was moderately diminished, but there was no chronic gastric catarrh, no ascites, and no enlargement of cutaneous abdominal veins. There was no tumour detectable in the abdomen. The urine was not albuminous. Neither polyp nor hæmorrhoids could be found in the rectum. The surface was blanched after the hemorrhages, but there was not a trace of cancerous cachexia. The diagnosis arrived at was: moderate degree of cirrhosis of the liver, conjoined with disease of the walls of the intestine, either ulcerative or cancerous in nature. At the autopsy, marked but not extreme cirrhosis of the liver was found. The stomach and intestines contained a large amount of blood, which had entered them through an opening about half an inch in diameter, close to the entrance of the ductus communis choledochus. A probe passed through this opening directly entered a cavity in the head of the pancreas, which was filled with fluid blood and clots.

The pancreas was referred to the Committee on Morbid Growths, who made the following report:—

The head of the gland, which is enlarged and hardened, presents in its interior a cavity about the size of a large walnut, which communicates with the duodenum by an ovoid orifice at least half an inch in diameter. The walls of the sac are trabeculated, but everywhere covered by a smooth mucous membrane, such as we often see in retention-cysts. There is but little of the proper gland tissue to be seen in this part of the organ, its place having been apparently usurped by dense fibrous tissue. The same state of affairs is also observable in the body and tail of the gland, although in a less marked degree.

The contents of the cyst had all been evacuated, but the microscope showed numerous crystals of hematin lying on the surface of its lining mucous membrane.

Thin sections under the microscope showed that the acini of the gland were diminished in size, the epithelial cells constituting their secreting structure being in a state of granular degeneration, and presenting an abundance of oil globules; and in some of the larger ones could be seen numerous crystals of margarine. The white and yellow fibrous tissue forming the stroma of the gland were increased in quantity, and evidently by their pressure contributed to the degeneration and atrophy of the secreting structure, aided, doubtless, by the pressure of the dilated duct.

After the fat was dissolved out by sulphuric ether, a small quantity of acetic acid was added, which brought out clearly the immense number of elongated nuclei existing in the fibrous tissue, and thus attested its vigorous nutrition and growth.

In view of these facts, the committee submits that the specimen is a well-marked case of cirrhosis of the pancreas, and believe that the cyst was a retention cyst caused by constriction of the excreting duct of the gland (duct of Wirsung); the constriction probably having its origin in some attack of subacute interstitial inflammation which had been attended by the exudation of lymph—such an inflammation as constitutes an essential part of the first stage of cirrhosis.

In view of the comparative rarity of pancreatic disease, and the numerous interesting but unsolved problems which that organ presents both to the physiologist and pathologist, the committee has been led to look up the scattered and not very abundant literature pertaining to its diseases.

Amyloid degeneration of the gland tissue has never been found, although it has been occasionally encountered in the vessels which supply it.

Syphilitic and tuberculous changes in the pancreas are very rare; cancer, however, is more frequent, and is usually primary.

Atrophy of the gland tissue and the formation of cysts, owing to chronic interstitial inflammation, are more common; and where cysts have been formed there is a marked tendency to hemorrhages, which are often profuse.

For a careful analysis of the subject, as well as of most of the well attested cases illustrating it, we beg leave to refer to the valuable Manual of Pathological Anatomy recently published by Prof. Klebs, of the University of Bern.

Nov. 10. *Tumour of the Lower Jaw.*—In presenting this specimen Dr. S. W. GROSS said:—

A. H., fifty-nine years of age, first noticed, ten years ago, a soft, fungoid mass at the gum of the lower right lateral incisor, which was decayed. On the extraction of the offending tooth the fungus shot up, and at the expiration of seven months, when it was cut away, it had attained the size of a small English walnut. The disease speedily returned, and, after the lapse of five years, it involved the alveolar border of the lower jaw, from the second bicuspid cavity of the left side to the corresponding point of the opposite side. A second operation was performed at this time, and the disease is described by the patient to have been of a cystic nature with gelatinous contents. Repullulation rapidly recurred, and a third operation, in which the gouge was freely used, and which was attended by free hemorrhage, was executed on the 10th of June, 1868, with a similar result.

The man was brought to the surgical clinic of the Jefferson Medical College on the 9th of November, 1870, when the jaw was found to be completely edentulous from repeated extraction of the teeth. The growth felt soft and elastic, and imparted to the touch a sensation of fluctuation. Its upper or free border consisted of thickened and very vascular mucous membrane, while its lower limit was formed by the basilar portion of the bone. The central part of the body of the jaw was particularly involved, being expanded and elongated at the mental process; but the disease extended apparently only as far back as the second molar alveolus of each side. It had never been the seat of pain, and there was no evidence of hereditary taint. The submaxillary lymphatic glands of the right side were slightly enlarged.

Professor Gross made an incision along the posterior margin of the basilar portion of the bone, which included the supposed limits of the disease; dissected up the soft parts, and divided the bone at the second molar socket with the forceps. It was then found that sufficient bone had not been removed on the right side, so that the entire body on this side, as well as the angle and an oblique portion of the ramus just above the insertion of the internal pterygoid muscle, required excision. The hemorrhage was excessively free, the blood oozing at every point, and showing no disposition to coagulate, even under the use of subsulphate of iron and the hot iron. A large number of vessels were tied; many bleeding points were included in the twisted suture; the edges of the incision

were brought together by harelip pins; cloths wrung out of ice-water were applied externally; while acetate of lead, tincture of digitalis, and opium were exhibited internally. Under the combined influence of these measures the oozing ceased in seven hours.

The tumour was composed of gelatinous material, of a greenish tint, and of the consistence of firm jelly. The mental process was perforated in front and behind by numerous openings, through which the same material was plainly visible. The mucous membrane of the gum, which formed the soft portion of the cyst, was greatly thickened and pervaded everywhere by minute vessels.

The tumour was referred to the Committee on Morbid Growths, which reported as follows:—

The tumour has probably had its origin in the medulla of the lower jaw in the neighbourhood of the symphysis, and having by its pressure caused absorption of the outer wall of the bone, has pushed forwards the thickened and inflamed periosteum, which now forms a dense cover for the exterior. At the symphysis there are a few small nodules of bone, formed by the inflamed periosteum.

Portions of the tumour placed beneath the microscope exhibit large spindle-shaped cells with well-marked nuclei and thin, fibre-like processes, often many times the length of the body of the cell. These processes are sometimes split at the end, and form a network anastomosing with neighbouring prolongations of similar cells; while others, placed side by side, form long bands, which give a trabeculated appearance to these portions. The cells are imbedded in a granular intercellular material, and some portions of the tumour present in abundance the so-called "giant" or "mother" cells crowded with nuclei.

The committee therefore believes the tumour to be a rapidly growing spindle-celled sarcoma (epulis sarcomatosa).

Syphilitic Disease of Testicle.—Dr. PACKARD presented this specimen. The history of the case was as follows:—

Mr. K., æt. 45, living in the interior of the State, a man of very robust frame, contracted a genuine hard chancre (as evidenced by the cicatrix on the left side of the body of the penis) four years ago. He had subsequently sore throat, nocturnal pains, and other constitutional symptoms, and the testicle became swollen a year after the sore appeared. The local trouble being occasionally very annoying and painful, and there being no other sign at present of any disease in the patient's system, I excised the testicle on the 4th of November, with the aid of Drs. Agnew and Sinkler. Healing took place very rapidly, and on the 16th he returned home.

It may be well to mention that I applied acupressure to the spermatic cord (the vas deferens being carefully separated), and removed the needle in forty-eight hours. To this, in part, I attribute the rapidity of healing.

The specimen was referred to the Committee on Morbid Growths, which reported as follows:—

The specimen of diseased testicle presented by Dr. Packard bears traces of repeated attacks of inflammation of the tunica vaginalis. Both the visceral and parietal surfaces of this membrane are coated with a thin layer of lymph, giving it on close inspection a somewhat trabecu-

lated appearance. At the position of the gubernaculum testis these bands are so developed as to resemble the fibrous expansions of the columnæ carneæ of the heart, as they pass to their insertion in the mitral valve. The epididymis appears healthy, although its fibrous envelope is thicker than usual. The gland proper is about the usual size, but on section shows at its central posterior portion a yellowish white nodule, ovoid in shape, $\frac{1}{2}''$ in its long diameter. Above this the secreting structure of the gland has undergone atrophy, and the mediastinum and the septula setting off from it are, on close inspection, to be distinguished from the dense whitish mass which has usurped the place of the proper gland tissue. Below the above-mentioned nodule the gland structure still exists; the septula, however, are thicker and more developed than usual. A section near the edge of the ovoid nodule, when placed beneath the microscope, shows a luxuriant growth of the cells of the connective tissue, with multiplication of their nuclei, and, as we advance towards the centre of the mass, these cells become enlarged and crowded with fat globules, although still retaining a definite cellular form; further on, however, breaking down into a mass of granular debris.

In view of these facts—the tough consistence, yellowish appearance and seat of the nodule, the disease involving only the testicle proper and not the epididymis; the thickening of the tunica albuginea, and the fibrous adhesions from repeated attacks of inflammation of the tunica vaginalis, together with the microscopic appearances which are so characteristically those of a gummy tumour—the committee has no hesitation in pronouncing the growth a typical case of syphilitic gumma of the testicle.

Cirrhosis and Complete Atrophy of Vesicular Structure of the Left Lung; Cheesy Deposit, with Miliary Tubercle throughout the Right Lung.—Dr. TYSON presented the specimens, and read the following history:—

John L., æt. about 26, a paid nurse in the men's medical ward of the Philadelphia Hospital, was said for several years to have had a left phthisis, and had cough for six years. He was, for some days before I saw him, under the care of the resident physician, in whose ward he had served.

On October 11th, when asked to see him, I found him with a hot skin, pulse 148, respirations 60, and a temperature of 102°.3 in the axilla. He stated that three weeks ago he was seized with chill and fever, with sharp cutting pains in the right side; has at present no pain except after a paroxysm of coughing, which is followed by acute pain under *left* nipple, but it subsequently passes away.

Percussion elicited dulness beneath the left clavicle anteriorly, and over the left scapula posteriorly, with corresponding bronchial breathing on auscultation. The left axilla was more resonant on percussion, as was also the base of the thorax, but occasional fine bubbling sounds were noted by auscultation in both situations. Resonance was greatly impaired throughout the right lung, and fine bubbling sounds prevailed correspondingly. In the right axilla these were accompanied by creaking friction. He was ordered to be dry cupped—first over the one lung, and on the following day over the other—with marked temporary relief. The usual restorative and stimulant treatment was directed. Little

change took place in the physical signs. He became gradually more feeble, and died of exhaustion.

Post-mortem examination.—The lungs alone exhibited phenomena of interest. Throughout the right lung were nodules of cheesy matter as large as a pea and larger; many of these could be expressed from small bronchial tubes. Equally disseminated, were translucent granulations of miliary tubercle, apparently of more recent date. The posterior part of the middle lobe of the right lung was the seat of a circumscribed pneumonia.

The left lung was interesting, in exhibiting complete absence of its vesicular structure, while the portion which remained was made up of numerous sacculated dilatations of bronchial tubes and intervening fibrous structure, so that the mass of lung remaining, which was flat and scarcely exceeded six inches in diameter, presented on section a perforated or honeycombed appearance.

Dr. WILLIAM PEPPER referred to a remarkable and somewhat similar specimen which he had presented to the Society in the early part of the year. It differed from the present specimen, however, as presenting the form of fibrous degeneration of the lung, attended with abundant formation of new fibrous tissue in the interlobular and interbronchial spaces, but without any marked dilatation of the bronchi. The lung was but moderately contracted, and on section presented a dense, marbled appearance of cartilaginous hardness, whereas, in the present specimen, the cirrhotic inflammation of the interstitial connective tissue has been followed by contraction, and consequent atrophy of the vesicular tissue, while the bronchial tubes have all undergone extreme diffused dilatation. The question is still open to debate, whether, in such cases, it is the cirrhotic contraction of the interbronchial tissues which draws open these tubes, or whether there is coincident inflammatory softening of the walls of the bronchi, which causes them to yield to the violent cough which attends such diseases of the lung. Probably both of these play a part in producing dilatation of the bronchi, when so extreme as in the present case.

REVIEWS.

ART. XVII.—*Traité Clinique et Pratique des Maladies Puerperales suites des Couches.* Par le Docteur HERVIEUX, Médecin de la Maternité. Première Partie. pp. 636. Paris, 1870.

A Clinical and Practical Treatise on Puerperal Diseases. By Dr. HERVIEUX, Physician of the Maternité. Part I.

AT mention of the term "puerperal diseases" the mind naturally reverts to but one of the class, one which in every respect, but especially in its fearful mortality, overshadows all the others. Puerperal fever is a disease dreaded not alone by the general public; its appearance as an epidemic is appalling to the profession; the stoutest heart sickens as the name brings to mind its wide-spread ravages, the peculiar interest which attaches to its victims, the powerlessness of our art, and pictures of the desolation of homes and the happy rejoicing of households turned to the bitterness of mourning.

Such a disease has of course attracted all the attention its importance demands. Its literature is very extensive, and has been enriched by the contributions of some of the ablest members of the profession during at least two centuries. Recent times have also shown in this, as in so many other departments of medecine, a renewed study of the subject; yet with all the records of the past, and with all the enlightened investigation of the present, there yet remain wide differences of opinion, much that remains unsettled, and important points yet reserved for the future to clear up. But most surprising of all is the fact that there is no unanimity of opinion as to the nature of the disease. It is yet keenly debated whether the disease is essentially a fever, or whether it is an assemblage of different inflammations; whether there is a general disease which precedes and determines the local lesions found after death, or whether the latter are initial and the cause of the general phenomena; whether, therefore, there is such a disease at all as puerperal fever, or whether the term should not rather be discarded, and each of the group of puerperal phlegmasiæ be distinguished according to its anatomical seat. It is not necessary for us here to range the authorities upon each side of these questions, to adduce their arguments, or to sum up the evidence; it will be sufficient, for the purpose of showing our own immediate interest in the subject, to call attention to the fact that two obstetrical authorities of our own country and our own time are upon opposite sides of these questions. Both of them occupied high positions as teachers; the text-books of both are in the hands of students to-day, and both have recently passed away, without, so far as we know, a shadow upon their reputations as honest men, careful teachers, and conscientious seekers after truth. Their failure to agree as to the leading point of the most important disease of the department of medicine to which they had devoted their lives, may help to bring home to us the difficulties which surround the question. Nor is the decision of it a matter of mere theoretical interest, and therefore of little consequence. Closely connected with it is the question of the contagiousness or non-contagiousness of the disease,

and more directly dependent upon it the principles of treatment, and beyond these points there can be nothing more interesting or more important to the practising physician. Of the two authors referred to above, the one who believes puerperal fever to be an essential fever is a contagionist, believing in the transmission of the disease by the accoucheur to other lying-in women whom he may attend; the other, who holds to the simple inflammatory nature of the disease, "utterly rejects and denies" this doctrine. In treatment, too, the localist follows his doctrines to their legitimate consequence under the teachings of the antiphlogistic school; hence full general depletion, repeated, if need be, is the chief therapeutic measure, essential and indispensable, and writing in a peculiar, pointed, and forcible style, he impresses the student with very uncomfortable feelings in view of a possible neglect of this remedy.

We need say no more to show the importance or the practical bearings of the unsettled questions relating to this disease. Very few of the profession anywhere are so situated as to be able to solve them from personal observation, very few have the time for a critical examination and comparison of authorities, whilst there are none who are not frequently, or may not have at any time, cases of the disease under care. For their benefit we propose to examine a new work upon the subject, simply to present the testimony of another witness upon some of the questions and matters relating to puerperal fever and puerperal diseases, but by no means attempting an exhaustive review of so important a class of diseases. We think it cause for congratulation that the witness is one so competent as Dr. Hervieux. His position has been such as to have afforded most abundant opportunities for observation, he has studied the subject with earnestness, he writes with clearness of what he has seen, and although he sometimes expresses his views with great warmth, we accept that as evidence of the strength of his convictions, and have found nothing in the pages of his work which seems to throw doubt on his candour or honesty.

The first section of the work is headed "On Puerperal Intoxication," and it opens, as becomes a complete treatise, with an historical and critical review of the principal doctrines which have held sway in the medical world as to puerperal diseases. This part contains some points which are far too interesting to pass unnoticed, and require no apology for introduction here. The first doctrine examined is that of the *suppression of the lochia*, which is the most ancient and the most continuous of any. It began with Hippocrates, 322 years before Christ, and continued down to Home, in 1772. As evidence of the manner in which the author has performed this historical part of his work, we may say that there are no less than thirty-five notes to as many different writers referred to, in this section alone, and of whose views a brief synopsis is given. We quote the result of his investigation of all these authorities :—

"1. The medical world has lived, during nearly two thousand years, under the doctrine of the suppression of the lochia.

"2. This doctrine has only begun to lose favour during the course of the seventeenth century, a certain number of authors no longer considering the suppression of the lochia but as one of the causes of puerperal diseases.

"3. That it disappeared entirely toward the end of the last century, to give place to others, and especially to the theory of lacteal metastasis, which we shall next examine."

Well may the author ask and exclaim :—

"Does there exist in pathology any other single doctrine, attaching to any

other part of our science, which comes to historical criticism with like title of nobility? A reign of twenty centuries—Hippocrates and Galen for first patrons—and the long series of illustrious names we have cited for defenders!"

He closes the retrospect with a brief statement of the results of clinical study of this point, which shows that the lochial discharge persists almost always during the earlier period of these affections, and is often remarkable for its abundance and fetid odour, that when suppressed it is generally at an advanced period of the disease, and hence the suppression should be considered as the effect, and not the cause, of the puerperal disease.

The doctrine of *milk metastasis* would seem, from what is said here, to have more sway in the author's country at present than with us, women even of the better class using the term. Its history begins with Sennert, in 1631; Primerose was one of its adherents, but Puzos, 1686, was its first great advocate. References are made to all the writers who maintained it down to Bichat, 1801. He first pointed out that the effusions in the abdomen were not milk, but pus, due to inflammation of the peritoneum, and were to be found as the result of peritonitis in non-puerperal women, and even in men: "The teachings of Bichat were the death-warrant of the doctrine of lacteal metastasis."

The author adds: "I would say in regard to the suppression of the milk in puerperal affections, that it neither takes place so soon nor so completely as has been held, and when it does take place must be considered, like the suppression of the lochial discharge, rather as the effect than the cause of the disease."

Article III. is upon the doctrine of *uterine and peritoneal localization*. Metritis was held to be the cause of puerperal fever as long ago as 1537, by Felix Plater, and the doctrine received the support of a long list of writers, down to Denman in 1768. In later times, in consequence of the progress of pathological anatomy, it underwent important modifications; uterine phlebitis and angeioleucitis were each for a time accepted doctrines, and each were maintained by some of the best names of the preceding generation of medical men. But

"The doctrine of metritis in whatever variety, whether as uterine phlebitis, angeioleucitis, or even inflammation of all the tissues and elements of the uterus, is very easily refutable.

"The whole question in fact is reduced to this: Is it true that inflammation of the uterus, or of its veins, or of its lymphatics, is constantly the starting point of puerperal diseases? Clinical observation distinctly answers—No."

Mead, in 1742, laid the foundation of the doctrine of *peritonitis*, and none of the more modern views can show such a list of illustrious defenders as this. Among them are Hulme, William Hunter, Leake, Bichat, and Pinel, while in the celebrated discussion at the French Academy of Medicine, in 1858, this doctrine was sustained by Cazeaux, Beau, and Velpeau.

"But whatever may be our respect for these distinguished men, it is impossible for us to give in our adhesion to the opinion which makes the peritoneum the exclusive point of departure of grave puerperal accidents.

"Is it necessary to repeat here that lying-in women may die of uterine phlebitis, of pleurisy, of erysipelas, etc., without presenting the least trace of peritonitis? Without doubt peritonitis is a very common occurrence; I have proof of it every day at the Maternity; but it is not general, and that is far from being constant."

The doctrine of *utero-peritonitis* was the result of a very natural fusion of the two of its isolated factors. Both being "issues of the organic

school—both hostile to the doctrine of puerperal fever," their union was inevitable.

"If this fusion renders the doctrine of metro-peritonitis more solid and more difficult of attack, it does not render it free from all objections. It is just as impossible for the partisans of this doctrine to render an account of the numerous cases in which the autopsy reveals only a pleurisy, a phlegmon of the iliac fossa, or a phlebitis of the inferior members."

The first traces of the doctrine of *puerperal fever* are found in Mercatus, 1570, and a long list of authorities, both French and English, are given of those who held or taught it, down to the closing decades of the last century, at which time there seems to have been no disagreement upon the subject, the only debate was as to its nature, whether it was inflammatory, putrid, or bilious. Several strong protestations, which are mentioned, were entered against this doctrine, but in spite of these the doctrine grew and flourished upon both sides of the channel, and reigned almost sovereign down to 1858.

"The question having been carried in that year before the Academy of Medicine, at the instance of Guerard, there was shown so wide a difference of opinion among the members of that learned body, that in spite of the eloquent pleas of Paul Dubois, Danyau, and Depaul, the cause of puerperal fever could not triumph. From that moment it has not ceased to lose ground in the minds of physicians, and it may be considered now as very seriously compromised."

The name, however, yet remains, and the doctrine is yet held by some; the author, therefore, enters into the consideration of the important question, "*Is there a puerperal fever?*" and we will try and follow him through his answer and present his arguments. It must be briefly, however, while we are obliged to confess that he is somewhat given to prolixity. The reader is not kept long in doubt as to the nature of the author's answers to this question, as he replies—

"*No, there is no such thing as puerperal fever*, in the sense ordinarily attached to this term, and I add, the admission of this seducing and convenient hypothesis is chaos, is a return to the infancy of our art; it is a negation of all diagnostic science, the obstacle to all therapeutic progress in puerperal diseases."

He then enters upon the examination of the chief arguments relied upon by the essentialists to sustain their doctrine.

1. *In some patients who have died with all the symptoms of puerperal fever the autopsy has not revealed any appreciable visceral lesion.*

The first reply to this consists in the results of his own experience. "In spite of the considerable number of autopsies which I have made at the Maternity, *I have never met with such cases.*" He insists, firstly, upon the difference there is between not finding organic lesions and their non-existence; claims that *post-mortem* examinations even conscientiously made and with the best intentions, are generally not minute enough; that it is not sufficient to open the cavities, but that the veins of the extremities should be carefully scrutinized, the pulmonary vessels and the large venous trunks examined, and a "luxury of precautions" taken, before it can be affirmed that there is an entire absence of organic lesion.

But admitting such a case or cases actually found, his views would not be changed, for

"I admit a puerperal poison, a poison which in different epidemics has different degrees of intensity, and consequently very diverse modes of action. Is there anything strange if this poison should attain such a degree of exaltation as to cause death in some hours, even before any morbid determination has been

produced? Are there not numerous poisons which, under certain determining influences, overwhelm the patient before they have had time to produce tangible alterations? This objection, then, does not destroy the theory which I maintain. But does it establish the unity of puerperal fever? The unity of cause—yes; but the pathological unity—I deny it. I deny it in the name of the diseases, so numerous and so diverse, to the study of which this work is consecrated, and which, for the most part, arise from the same etiologic principle, the puerperal poison, as in an army decimated by diseases, the typhus, the dysentery, typhoid fever, erysipelas, hospital gangrene, etc., originate from one same toxic principle, the miasm of camps."

2. *It has been claimed that puerperal fever depends upon an alteration of the blood, and that upon this account it should be classed with the pyrexias, among the typhus fevers.*

This alteration of the blood, when it exists, the author claims may result from phlebitis, either uterine or of the lower extremities, from pelvic suppuration, from peritonitis, or purulent or putrid infection. He admits the alteration as in harmony with the doctrine of puerperal poisoning, but the numerous diseases arising in consequence of this poisoning are no more identical than are the various diseases arising from camp or hospital miasm, all of which are also accompanied by an alteration of the blood.

3. *An identity of symptoms observed in peritonitis, uterine phlebitis, purulent infection, suppuration of the pelvic veins, etc., has been invoked in favour of puerperal fever.*

His answer to this is simply a denial of the identity. That several forms of disease may coexist, he admits, that care is not taken, frequently, to distinguish between them is certain, but he is also certain that generally they can be clearly distinguished during life. Between peritonitis and uterine phlebitis the differential diagnosis is comparatively easy. "Every day we make, clinically, the distinction between these two affections, and with certainty almost mathematical."

4. *One same very general cause has produced very diverse lesions, but finally there has pre-existed something general, engendered by a specific agent, unknown in its essence, and which has penetrated by infection or by contagion.*

The existence of this specific agent, miasm or poison, is admitted; its entrance by infection or contagion is also admitted. The comparison of the diseases of armies arising from one miasm is again made and pushed: "From the identity of cause the identity of nature cannot be deduced."

5. *The partisans of puerperal fever admit three distinct forms of this fever—the inflammatory, the mucous or bilious, and the typhoid form.*

These are based upon an inexact appreciation of the various puerperal inflammations, and can all be better explained by them—the inflammatory corresponds, for instance, with simple metritis; the typhoid, with purulent infection.

"I can comprehend how this hypothesis of three well-defined forms of puerperal fever can be perfectly acceptable to practitioners who have never had occasion to make a post-mortem examination of a lying-in woman; but for men who have been enabled to observe the cadaveric lesions of the puerperal state, and to compare them with the constant phenomena during life, to seize consequently, the clear and palpable relation, the logical cord which unites them, I cannot conceive the obstinate attachment of these to an undermined doctrine, and one which reckons among true *clinicians* only a few adherents."

The theory of puerperal fever has had a bad influence upon the progress of medical science by its indefinite diagnosis; upon therapeutics the effect has been still worse.

"It is difficult to estimate exactly the injurious influence which certain words can exert upon the progress of our art. It is under the influence of the name puerperal fever that a crowd of physicians have been led astray, and have imagined the numerous and illusory therapeutic methods which have been proposed against this pretended morbid unity. If, in place of having striven to combat a puerperal pyrexia unknown in its essence, illy defined in its attributes, practitioners had directed their efforts against the pathological realities which manifest themselves after parturition; if they had sought the best modes of treatment for puerperal peritonitis, general and circumscribed; for uterine phlebitis, simple or complicated with purulent infection; for all the varieties of pelvic suppuration which, with a little skill and attention the experienced physician is able to recognize and distinguish, the therapeutics of the puerperal condition would not have undergone so many fluctuations; would not have been shuttle-cocked between so many plans of medication diverse and too often contradictory; it would not have been delivered over to all the hazards of suggestion, and most generally of unfortunate suggestion."

The next chapter is upon the doctrine of "*puerperal traumatism*," a doctrine founded upon the comparison of a lying-in woman with one who has undergone amputation; the still open vessels of the uterus at the placental attachment are then the channels through which passes the purulent infection, and from the unhealthy suppuration of which arises traumatic fever, etc. Considering the position, character, and standing of some of those who have supported this doctrine, it is very briefly disposed of; yet we must admit that the argument against it is unanswerable. Admitted to be true in a certain number of cases, it fails to include those cases frequently observed in every hospital, in which the symptoms begin during or even before the occurrence of labour. It must be rejected, therefore, as unsatisfactory because not including and accounting for all the facts.

It will already have been seen that the author is an advocate of the doctrine of a *plurality of puerperal affections;* and with a sketch of the history of this doctrine he closes this branch of the subject. Traces of it are found in medical writings for more than three centuries, and it has had occasionally some strong supporters, but the doctrine of puerperal fever overshadowed it or kept it in the background. This doctrine alone he believes to be in harmony with all the facts of the case, to be free from the objection of indefiniteness, practical in regard to therapeutics, and, finally, demonstrable by autopsy. Referring again to the varied diseases of military life arising from a single source, and from the multitude of diseases which everywhere arise from the single cause of exposure to cold, he concludes:—

"Yes, there is a puerperal poison, a miasm of lying-in hospital, which, like the miasm of camps and of surgical wards, can engender numerous and very diverse affections; affections which, although arising from the same cause and having the same origin, are none the less morbid entities essentially distinct."

That there is a wide difference between this poison, a pathological fact and the puerperal state, which is a physiological condition, he insists, although they are often confounded; he also insists that the traumatic conditions of labour will not explain the fact that in numberless country districts the mortality of lying-in women is less than one per thousand, while in other localities it rises to fifty, sixty, or even one hundred and twenty per thousand. This can only be accounted for by a miasm, "the product of the vitiation of the atmosphere; by the physiological or morbid, but especially by the morbid, secretions of lying-in women," and which we

know only by its effects. That these effects are various and multiple is analogous with other occurrences, as we have seen, and dependent upon causes beyond the reach of one's investigation. That they do vary most widely is, however, frequently shown; sometimes simple metritis is the only affection, sometimes peritonitis, sometimes uterine phlebitis, and then again there are periods during which only pleurisy occurs.

"It is by virtue of this special epidemic influence that I have seen entire years pass at the Maternity without meeting with a single case of varicose phlebitis, in spite of the large number of pregnant women afflicted with varicose veins who are admitted. In other years, on the contrary, as in 1862, almost all those confined having varicose veins were attacked with superficial phlebitis of the inferior extremities."

This puerperal poison is not, however, confined to lying-in hospitals, although most frequently seen in them, because in them are found the conditions eminently propitious for its origin. Any lying-in woman may give rise to it, where certain disturbing influences come in addition to the ordinary traumatic effects of labour, and she may inoculate herself or become a centre from which the poison is disseminated. These auto-inoculations are necessary to explain many facts and harmonize them with the general doctrines of the subject.

We scarcely expected to find any one at this late day claiming the doctrine of puerperal poisoning as something new, and a personal discovery! We should, least of all, have expected to find such a claim made by a writer who quoted upon every page, the best authorities upon the subject in our language! Yet the following extract certainly looks like it! He is writing of the uncertainty in which he finds himself, when first trying to comprehend and clear up the subject from actual study at the bedside:—

"But a day came in which light broke through. The solution of the problem appeared to me clear, easy, and marked by evidence the most irresistible. One word, a single word, had sufficed to dissipate all the obstacles which enveloped this question of puerperal epidemics, so shadowy and so complex. This word is *poison*. There is a puerperal poison, and there is then a puerperal poisoning. Of this poisoning I must make known the numerous manifestations, the multiple and varied lesions, the pathogeny and the prophylaxis. To the accomplishment of this task, I have applied myself, and I believe that I have attained this end as completely as possible with the means of investigation at present in our possession."

How a man familiar with the literature of the subject could write as follows seems incomprehensible:—

"Guided by the doctrine of poisoning as by a torch, we have been able to ransack all the innermost recesses, thus to speak, of the pathology of lying-in women, to decide many questions until now without solution, and in all respects carry some useful contributions to the history of the unexplored part of our subject."

Looking carefully over these doctrines as to the puerperal poison and the diverse diseases it occasions, we must confess that the difference between the author and those who hold to the doctrine of essential fever, which he combats so energetically, is not so very wide after all. A general disease with local manifestations; just as diphtheria is a constitutional disease, while the local ailment is generally in the throat, but may be elsewhere, so the local manifestations of puerperal disease are generally in the pelvis or abdomen, but may be elsewhere. That the difference is one of words rather than of facts—of names than things—may be seen from the follow-

ing extract, for instance, from Dr. Kennedy, of Dublin, who holds puerperal fever to be a "zymotic disease par excellence."

"I shall, therefore, classify the whole family under the head of "Zymotic Metria," in preference to that by which it has been hitherto received, of puerperal fever; and under this common head I shall treat, as modifications of the same disease, puerperal fever, arthritis, pyæmia, purpuric or cerebro-spinal metria, traumatic metria, erysipelas or hospital gangrene. This I feel justified in doing, because I have traced one and all of them to the same contagion."[1]

We pass over entirely the chapter upon the history of puerperal epidemics to continue the study of the puerperal poison in its etiological aspects. The author divides the causes into general or determining and individual or predisposing. The latter are 1, physical distress; 2, moral distress; 3, acclimation; 4, constitution and antecedent diseases; 5, primiparity; 6, obstetrical influences.

Under the first head is drawn a sad picture of the misery and destitution of the poor of Paris, and of the sad fate of those whose lapse from virtue has driven them from country districts to hide their shame in the crowded solitude of a large city. But the picture does not belong to Paris alone; it belongs to every metropolis; nor has the author alone shown the evil influence of want of food, lodging, and clothing upon parturition.

Under the second head he bears the strongest testimony to injurious influence of the depressing emotions, and many writers have recorded their experience in almost identical terms.

"I have seen many times in my service young *accouchees*, in progress of convalescence, take a chill and become mortally ill, in consequence of a visit and violent reproaches made by a mother or an injudicious relative.

"I have seen unmarried mothers, until then doing well, in consequence of the agitation and the perplexity caused by the resolution to give up their child, fall ill the day after the execution of this resolution, and succumb within a short time after."

Acclimation refers to the influence of the time of residence in the hospital, previous to confinement, upon the mortality. The author quotes conflicting testimony upon this point, which induced him to investigate the subject anew. We cannot give his tables nor details, but the result is, that during the period of ten years "the percentage of women dying after having sojourned in the hospital some time before their accouchement is notably higher than that of women confined on the day of their admission."

There is no room for doubt as to the evil influence of the primiparous condition. The author's experience is in accordance with that of all the other authorities he quotes. Of 190 fatal cases of puerperal diseases occurring under his observation, 119 were primiparous, and 71 multiparous.

Long duration of the labour was recognized as a cause of puerperal accidents by Mercatus three hundred years ago, and modern observation has confirmed the truth of the observation. By the author a long labour is looked upon as the predisposing cause, *par excellence*. Obstetrical operations are also shown by numerous quotations to exert a marked unfavourable influence. It will not do, however, as the author shows, to take the bare figures of statistics upon this point; for, comparing them with those of the general mortality, it is found that they rise and fall together, and he introduces a diagram to show the symmetrical changes and thus strikingly illustrates the powerful influence of the hospital for evil, since

[1] Dublin debate upon Hospitalism and Zymotic Diseases; abridged report.

the majority of the operations are such as are frequently performed in the country, and without adding, or very slightly, if at all, to the mortality.

The study of the general causes is far more important for the discovery of the law which governs the origin of puerperal epidemics, and we must not seek for this law in the lying-in woman herself, but outside of her, in the medium in which she is placed, or in the persons or things surrounding her. The first in order of these is meteorological conditions, which exercise an undoubted influence upon the existence of puerperal epidemics, as shown by the statistics from Vienna, Dublin, St. Petersburg, and London, as well as Paris. Here, too, it will not do to read the figures alone. During the cold and wet seasons of the year, when the mortality is increased, other influences are at work to complicate the problem; at those times physical distress is increased, the hospitals are more crowded by the greater number driven into them, and ventilation is not so efficiently carried out. The tables, however, show a sufficient increase of the death-rate to prove the influence of the certain seasons after making allowances for all these things.

Vitiation of the air by the secretions of lying-in women, often in the most normal condition disgusting, and always eminently putrescible, needs but to be mentioned to be recognized as one of the chief causes of the disease. Besides the gaseous products of their decomposition, recognizable by chemistry, there are undoubtedly also present specific germs or sporules capable of engendering or propagating diseases. Full belief is accorded by the author to the existence of such bodies, and the recent researches of Pasteur, Reveil, and others adduced in support of it.

"From these researches we may, with Trousseau, conclude that there is produced in the air, at certain times, and under conditions which the future will determine, morbid ferments which will permit us some day to seize the cause of endemic and epidemic diseases."

It would seem, at the first sight, like a work of supererogation, to set forth the evil influences of overcrowding upon the inmates of lying-in hospitals. Yet very high authority can be quoted in favour of the opinion that the number of inmates has no influence upon the rate of mortality. Prof. Spaeth, for instance, of Vienna, has shown that in the long interval between 1784 and 1863, the years in which the death-rate has been the highest have not been those in which the hospital has been most crowded, and *vice versâ*. The author admits that the same may be said of the Maternity of Paris from 1802 to 1864. The doctrine also found advocates in the Dublin debate upon the subject, although the opposite opinion certainly had the greater weight of authority. The author meets the facts given above by saying that they only show the causes of puerperal disease to be multiple, and that we do not know them all. He then goes on to show that in the Maternity of Vienna, during the period named, the mortality did not ascend above 4 per cent. while the population remained below three thousand; so soon, however, as this number was passed, in 1819, it showed a decided increase, rising in 1837 to 10.4, and in 1841 to 16.9 per cent.

"Consult the statistical tables of the Maternity of Paris from 1802 to 1863, and you will remark that in proportion as the figures of the population increase those of the mortality increase in like proportion."

In further confirmation he quotes the statistics of Bristowe and Holmes, showing the relative mortality of large and small hospitals. His argument upon a point so generally understood as this it seems to us need not

have been so laboured; he shows here again too much effort to refer the prevalence of the disease to known physical causes, and great reluctance to recognize the epidemic influence as a powerful factor, and yet beyond our ken.

By infection the author understands "the pernicious influence of air charged with putrid miasm." The air of a lying-in chamber or ward may be very offensive, and yet no infection result; another element is therefore wanting. He finds this in the continuity of the cause. This it is which gives rise to the most disastrous epidemics of puerperal diseases, and this it is which makes disease endemic in certain hospitals, so that for many years in succession it is never absent—the very walls become infected.

The subject of contagion is as closely allied to infection as are the words, as generally used. As more in accordance with the derivations we should have preferred to consider some points under the preceding head which we find under this latter, limiting this to transmission by actual contact. But the author opens with a lengthy comparison of the mortality of lying-in hospitals, with accouchements outside of them. Statistics are given of most of the leading cities of Europe, taken in great part from Bristowe and Holmes, Le Fort, Tarnier, and other sources already before the profession. They all accord with the following by Malgaigne, from the Official Bulletin of the Minister of the Interior, made up from the returns of the two years 1861–2: Mortality of lying-in women in hospitals 8.2 per cent., in the city and *bureaux de bienfaisance* 0.5 per cent.; and by Hugenberger, of St. Petersburg, for fifteen years, mortality of hospitals 4.3 per cent.; of civil practice only .66 per cent.

"It is vain to attempt to invoke any other cause than infection and contagion to explain this enormous difference which exists constantly and everywhere between the mortality of women confined in hospitals and at home. I defy the most consummate skill to overcome the differences with the subtleties of dialectics, and thus to succeed in putting error in the place of truth.

"Direct proofs of the reality of contagion are not wanting, and it would be superfluous to adduce them. The belief in contagion is in fact universal. There is not a capital in Europe in which the medical public does not partake of this belief. Paris, it must be admitted, has remained a long time behind the other scientific centres upon this question, but to-day there is not one of us who does not, unwittingly perhaps, talk and act as if he was convinced of the power of contagion."

He quotes and indorses the statement of Bristowe and Holmes in their report upon the hospitals of Great Britain that isolated cases of puerperal disease, "by the fact alone that they are isolated," do not tend, like large numbers of sick together in times of epidemics, to engender an infectious principle and become contagious:—

"This distinction between the non-contagiousness of sporadic puerperal accidents and the contagiousness of the epidemic diseases is of the highest importance, both practically and theoretically. Theoretically it ruins the last entrenchment behind which the non-contagionists take refuge when they invoke, in defence of their doctrine, the absence of any well-authenticated case of contagion outside of a time of epidemics. Practically this distinction, firmly established, relieves us when there is no epidemic prevailing, from those numerous and vigorous precautions which prudence commands at such times."

We do not find, however, any of those precautions anywhere detailed, or alluded to, which we should have expected in a complete treatise by one who expresses so strongly his belief in contagion. We refer to the personal precautions on the part of the accoucheur.

Contagion by the agency of a person recently engaged in post-mortem examination is duly noticed, but we find no allusion to the danger of the practitioner going from attendance upon cases of erysipelas to the lying-in room. Indeed we do not find, in this portion of the work, any reference to a probable connection in any way, or similarity of, the two diseases. This is singular when compared with the very strong opinions expressed by some of the most eminent men of the Dublin school. Dr. McClintock, for instance, maintains the common nature, the *identity* of zymotic metria and traumatic erysipelas to be established.[1]

The different modes of transmission are then considered, from a diseased *accouchee* to a healthy *enceinte*, of which the Maternity offers dozens of instances each year, violent rigors and abdominal pains occurring before the rupture of the membranes, even before the first uterine contractions—from a diseased *accouchee* to a healthy one—from a diseased *accouchee* to a non-puerperal subject. More important than these to the general practitioner is contagion by the medium of a healthy person, against which, we are told, Paul Dubois has expressed himself energetically. Nevertheless, abundance of authority is cited to sustain it: Gooch, Gordon, Ramsbotham, Blundell, Jackson of Philadelphia, and many French authors are quoted, and the particulars they give mostly detailed. We do not find here the original instances we should have been glad to quote as confirmatory additions to the evidence upon the subject.

Prophylaxis follows in natural order after etiology, and the measures to be put in force are both apparent and effective in proportion to our knowledge of the causes. First and most unfailing among preventive measures stands the accouchement of women at their own homes. But, as the author sagely remarks, in order to do this, it is necessary that they have homes! and under the head of "physical distress" he has shown that five-sixths of those who come into the hospital have none! Within a few years past a plan recommended by Le Fort has been adopted, of sending women to be confined at the houses of such midwives as were in a condition to receive them, and the results have been excellent. From the diminished mortality, the author argues, again, against a general epidemic cause, and in favour of dependence of puerperal disease upon the bad sanitary condition of a locality. But there are instances of epidemics prevailing over wide-spread districts in the country, and raging in a new hospital only just opened, as at Munich, which, we think, sadly interferes with his argument.

The other measures of prevention relate to the management of hospitals. When the disease has become epidemic, complete evacuation of the whole building for a definite and reasonable time is insisted on; no half-way business is tolerated—the evacuation must be radical.

Wide spacing between the beds is indispensable to a satisfactory sanitary condition.

"The architects, preoccupied solely with the cubic measure of air, have thought that by considerably increasing the height of the wards their salubrity would be increased in an equivalent proportion. This is an error which is sufficiently demonstrated by the elevation of the wards of the hospital Lariboisière,

[1] As an instance of what appears to us the most unnecessary befogging of questions of this kind we cannot forbear alluding to the fact that one of the strongest advocates of the identity of nature of erysipelas and puerperal fever, in the Dublin debate, after tracing their similarity in almost every respect, closes by denying the spontaneous generation of puerperal fever! Certainly in no other point do the two diseases more resemble each other than in the facts of their sporadic or epidemic occurrence.

which do not give a less mortality than the low and narrow wards of the more poorly built hospitals."

Looking upon a diseased lying-in woman as a centre from which infection is radiated in every direction, elevation of the ceiling cannot diminish the horizontal influence, which is the most important, because directly affecting others.

The alternate occupation of beds and wards is shown to be one of the most efficient preventive measures by the results obtained at the Rotunda of Dublin, where the system is in practice. To this plan, together with but few patients in the same ward, Scanzoni at Würzburg and Schwarz at Göttingen attribute their low rate of mortality, which was, during five years, for the former 1.2, and for the latter 1.1 per cent. A table is given of the mortality of the Maternity of Vienna for nine years, showing that immediately after Professor Spaeth took charge of the institution, putting in force this plan, together with opening the windows, the death-rate fell "for the first clinic" from 4 to 3.8, and then 1.4 per cent.; and "for the second clinic" from 4.8 to 2.6, and then to 0.5 per cent.

Ventilation, thorough and continuous, the abolition of curtains, cleansing of wards and furniture, immediate separation of those attacked with puerperal disease from the healthy, are measures obviously necessary, and such as would occur to every mind. We close with the general principles which the author lays down for guidance in the construction of lying-in hospitals:—

"Erect establishments destined to contain not more than from forty to sixty inmates.

"Let them consist of three separate buildings, one to be appropriated to the pregnant women, one to the lying-in, and one to the lying-in who are sick.

"Connect these buildings by galleries, and dispose them in such a way as to intercept neither air, light, nor the salutary action of winds and rain.

"Divide each building into wards for six or eight beds; eight for the pregnant, six for the *accouchees*.

"Separate the beds of the *enceinte* by a space of a metre and a half, and of the lying-in by two metres [6.56 feet], at least.

"The wards should be so built as to insure a natural ventilation as free as possible.

"Each ward should have two exposures, and an equal number of windows in each of them.

"Each building should have but two stories, the upper one to be occupied by the nurses."

In a succeeding number of this Journal we hope to present the author's teachings as to some of the individual puerperal diseases. J. C. R.

ART. XVIII.—*Histology.*

1. *Handbuch der Lehre von den Geweben des Menschen und der Thiere. Unter Mitwirkung* von J. ARNOLD, E BRÜCKE, COHNHEIM, RECKLINGHAUSEN, MAX SCHULTZE, und Anderen. Herausgegeben von S. STRICKER. I. und II. Lieferung. 8vo. ss. 428. Leipzig, 1869 and 1870.
2. *Manual of Human and Comparative Histology.* Edited by S. STRICKER; assisted by J. ARNOLD, E. BRÜCKE, COHNHEIM, RECKLINGHAUSEN, MAX SCHULTZE, and others. Translated for The New Sydenham Society, by HENRY POWER, M.B., etc. Vol. I., 8vo. pp. 600. London, 1870.
3. *Report to the Surgeon-General U. S. Army, on Certain Points connected with the Histology of the Bloodvessels.* By Bvt. Lieut. Col. J. J. WOODWARD, Assistant Surgeon U. S. Army. 4to. pp. 8. Washington, D. C., 1870.
4. *On the Cellular Structure of the Red Blood Corpuscle.* By Jos. G. RICHARDSON, M. D., Microscopist to the Pennsylvania Hospital. Proceedings of the American Medical Association. Vol. 21, 1870.

FOR a number of years past the student of histology, especially in England and this country, has been at a loss for a reliable systematic handbook. Conscious of the numerous additions constantly made to our existing knowledge of the subject, and of the total revolution which modern aids to research were producing, he has rightly considered it unsafe to rely upon the latest editions of Kölliker, and Todd and Bowman, which had always been the accepted authorities with English and American students. Of the former there is no more recent English edition than that of 1860, and the latter, at the time of its issue so excellent, has not been revised—except as to Part I. by Dr. Beale, in 1866. Even in Germany the last edition of Kölliker, 1867, dates prior to many of the most important researches of Cohnheim, Recklinghausen, Stricker, Max Schultze, Brücke, and others which are scattered throughout periodical literature, or are contained in monographs. The French have had the advantage of Robin's recent editions.

The need, therefore, of a work upon histology at this time is palpable. It is scarcely necessary to state that for any single individual, practically to investigate the whole domain of histology is impossible. Yet it is equally impossible for the student to look up monographs upon different subjects, scattered through journals often inaccessible, to say nothing of the amount of labour involved, or the difficulty in discriminating among conflicting views. To combine, however, the papers of the ablest investigators on special subjects in a single publication, under the direction of one who is thoroughly familiar with the whole ground, and at the same time a practical worker—being himself a contributor upon special subjects—is the attainment of an end most to be desired.

The subject is appropriately introduced by Stricker himself, in a valuable paper on General Methods of Investigation. The reader will find in it much valuable information as to the methods pursued in Germany, of which many are, however, too complicated to admit of general use among us,—divided as is our attention by the varied engagements incident to American systems of practice and teaching. Few can think of taking advantage of such delicate arrangements as those of Schultze and Stricker

for the warming of the stage, or of the arrangement of Brücke and Stricker for the application of electricity to structures *during* microscopic investigations. Until laboratories for physiological, pathological, and chemical research are furnished us, with well-salaried directors, we must be debarred facilities which seem essential to the successful study of the true structure and of the processes of formation of normal and diseased tissues.

Difficulty is also encountered by the American student in the application of the facilities suggested in this chapter of Stricker, by the use of appliances and instruments which must evidently be familiar to German readers, but which would require further explanation to be understood by the American student. Thus, "Brücke's doublet" and the lenses of Steinheil aus München, recommended for special modes of investigation, are doubtless excellent for the purpose, and well known to German and even American students abroad; but their peculiar construction is known to but few of us. Such minuteness, however, could not be asked in an introductory of thirty-six pages. The fact simply renders the chapter disappointing to the English student, as to the amount of practical information he will obtain from it—a disappointment only increased by the knowledge further gathered that we are so markedly deficient in what seem but everyday facilities to the German workers.

We notice that the term "direct" light is used in all instances where we are in the habit of using that of "reflected light," though there is no possibility of misunderstanding its application to one having read the text of the first pages. The words "transmitted light" are used, of course, in our own acceptation of them.

We believe Stricker correctly puts it when he says:—

"I can attribute no very high value to the binocular (double tubed) stereoscopic microscopes, so far as their use has at present extended. As yet they have only been employed with low powers. The relief of different parts of an object can be very well ascertained, even with a simple microscope, by merely varying the inclination of the head."—*New Syd. Soc. Transl.*, p. vi.

The chapter is accompanied by illustrations, which materially aid the understanding of the apparatus, and without these, indeed, the often complicated nature of the latter would sometimes be confusing.

But it is particularly in connection with the detailed consideration of the different tissues that these general considerations become valuable. We pass, therefore, to Chapter I., on *The General Characters of Cells*, which is also written by Stricker himself, and treated in a masterly manner.

Beginning at the period (1835) at which, under the leadership of John Müller, biologists ascribed to the microscopic constituents of the body a preëminently inferior role, in their dependence upon the influence of the vessels, the history of the cell is traced from that date to the present. At this time the independent existence of the vegetable tissues was admitted, and the first step consisted in proving the analogy of the two kinds of cells, accomplished by Müller himself, Valentin, Henle, Schleiden, and finally, Schwann. The comparison previously made by Virchow, of the whole organism to a free state, containing individuals endowed with equal privileges, and the unreserved acceptance by Müller of Schwann's proposition, resulted in a complete change in the views as to the physiological significance of the cell. The study of the lower forms of animal life, of which one of the results was the discovery by Dujardin of sarcode in 1835; the observation of Siebold of the movements of the vitelline spheres of the egg of the Planariæ, and the study of these phenomena by Kölliker,

Virchow, Leydig, Meyen, Huxley, Max Schultze, and Müller; the comparison of sarcode and the proof of its identity with the protoplasm of animal and vegetable cells, by Max Schultze, W. Kühne, E. Brücke, and E. Häckel, further extended these ideas, which have thus found expression in the language of Brücke:—

"If we consider how complicated the mechanical arrangements must be which lie at the root of the spontaneous movements of cells, and if we consider further that up to the present time we have only paid attention with the microscope to obvious and perceptible movements, and that no regard has been paid to the arrangement by virtue of which this little organism nourishes itself, increases its size, and begets its like, nor any to those means by which it displays its specific attributes; if we, I say, consider all this, we must recognize that we have to deal here (in the cell) with an organism, the complications of which, although truly not comparable with that of an animal, nor affording any good reason for believing that it is itself composed of innumerable small organisms, yet constitutes one to which we may fairly attribute the possession of a highly artificial structure, the essential architectural elements of which are completely beyond our grasp."—*New Syd. Soc. Transl.*, p. 4.

The ideal type of a cell, as thought to have been demonstrated by Müller and Schwann, and so long admitted, though not undisputed, was first abandoned, on sure footing, by Leydig in 1856, who maintained for the contents of the cell a higher dignity than for the membrane, constituting the material basis for sensible and irritable processes; and who maintained also that the conception of a cell only requires the existence of *a little mass of substance* (*klumpchen*) *inclosing a nucleus*, while the cell wall is only the hardened external layer (p. 4). Even Schwann did not claim for the corpuscles of mucus and pus any distinctly demonstrated membrane, but regarded them as minute round masses containing a nucleus, which might be termed cells, because this is the elementary form of all animal and vegetable cells. There might also have been added Schwann's own words, that "many cells do not exhibit any appearance of the formation of a cell membrane, but they seem to be solid, and all that can be remarked is that the external portion of the layer is somewhat more compact."—*Syd. Soc. Translation of Schwann's Researches*, p. 176.

Max Schultze is, however, accredited with having first, in 1861, effectually overthrown the idea of the vesicular structure of cells, and also having previously shown the absence of a cell wall in many instances, he defined the cell, as a *mass of matter* (*protoplasm*) *surrounding a nucleus*. The essential point in this definition is that the identity of the so-called cell contents with the animal sarcode was clearly recognized.

Though Pringsheim had shown, in 1854, that a primordial utricle does not exist in the vegetable cell, and Remak had already applied the term "protoplasm" to the animal cell, it was not until Max Schultze proposed to apply the term to the *living* mass of the cell that it came into general use. According to Schultze, also, "a distinct membrane may, indeed, appear on the surface, *formed by the conversion of the outer layer of protoplasm, but then it must be allowed to be an early indication of a retrograde process.* A cell invested by such a membrane can no longer divide; that is a power possessed by the enclosed protoplasm alone. A cell with a membrane differentiated in its chemical characters from the enclosed protoplasm, is like an encysted infusorial animalcule." Any one familiar with the theory of Beale will not fail to see the practical identity of his views, independently worked out, with those of this eminent and accurate German observer.

Brücke goes farther in asserting that there is no proof that even the nucleus is indispensable to our conception of a cell, resting his statement on the fact that cells are known to exist in the cryptogamia in which no nucleus is visible.

Stricker, also, adducing the non-nucleated amœba discovered by Schultze, the non-nucleated protista of Häckel, and the non-nucleated monads of Cienkowski, agrees with Brücke, and concludes that "if we do not desire to advance the statement that the non-nucleated bodies of the lower plants and animals, and the fertilized ovum, occupy a unique and isolated position which is not assumed by any other being in the whole scale of creation, we must exclude the nucleus as an unnecessary factor in the ideal type of an elementary organism."

We have elsewhere expressed our conviction with regard to the correctness of these views of Brücke and Stricker. If observation is the basis of the idea of a correct type, they are beyond controversion.

We cannot, however, if we interpret them aright, admit as logical the author's conclusions with regard to certain little masses of protoplasm, destitute of nuclei which present some of the phenomena of life, and certain fragments or minute separated particles about the size of the nucleolus, which, if they become attached to the slide, exhibit lively movements, especially if the object plate be warmed to from 68° to 70° Fahrenheit. To these, he says, we are not justified in giving the name of cells any more than we can apply the name of the whole of the animal to the excised heart of a tortoise. But we contend that as these exhibit all the characters described as constituting the cell, they should not be excluded. We deem this also necessary on the part of the author himself, to secure consistency with his paragraph immediately succeeding the above quotation:—

"We must, also, in future, apply the histological term cell to the morphological elements of the higher animals, or to independent living organisms, even if we are unable to discover anything more in their structure than that they are little masses of animal sarcode or protoplasm."

Under the section, *physiological peculiarities of cells*, contractile substance or protoplasm is said to be, when examined with the best microscopes, homogeneous or destitute of structure. Here, again, with due respect to so eminent an authority, we consider such a description applied to the white blood-corpuscle, or pus, or mucus-corpuscle, as inaccurate, though it correctly indicates the structure of the amœba the type of the protoplasmic substance.

Protoplasm may be said to be solid, fluid, or gelatinous, and is termed living because it exhibits the sum of phenomena which we have learned by experience to be characteristic of living animals, active or spontaneous movement, nutrition, and growth, and the capacity of producing its like.

Phenomena of Movements in Cells.—These are concluded to be present either from the visible movements of granules imbedded in protoplasm, or from the occurrence of certain changes in form of the protoplasm itself. The former is, of course, a passive movement.

In the movements of granules is included the vibratory, molecular, or Brunonian movement, seen in the granular contents of the salivary corpuscles, and under certain conditions in the colourless blood-corpuscles, pus-corpuscles, and other cells. It is stated, however, though we do not think with sufficient stress, that these do not depend on the vital properties of the protoplasm, since they take place in dead cells, and,

indeed, under any circumstances within or without cells where the consistence of the menstruum admits it, and they are not relatively too heavy. We do not think it logical, however, to admit the identity of two bodies, as the salivary corpuscle and white blood-corpuscle, simply because their granular contents exhibit the molecular movement, which is a matter of pure physical relation between the granule and the menstruum in which it lies. If such reasoning be admitted, even more justifiable would it be to declare the two cells are different because a ½ p. c. solution of chloride of sodium causes the molecular movement in salivary corpuscles to cease, while it does not affect that in the interior of pus or lymph corpuscles. The *progressive* movement of the granule, however, which is passive, depending upon the bodily movement of the cell substance, is, as stated by Stricker, an evidence of the presence of life, though such movement as this is also known to take place in matter admittedly devoid of life.[1]

Stricker further states (p. 15) that "these alterations in form only, which may be perceived in the object when the field of view is stationary, and when the object is adherent to the slide, and which are frequently repeated, enable us to determine the presence of life in it." Nor must we consider a protoplasmic mass of matter necessarily dead, even if we are unable artificially to excite movement by means of reagents.

But protoplasmic corpuscles are not only able to change their form, they can also *wander*; and this is accomplished by the protrusion of a portion of the mass which drags the rest after it. This was first shown to be the case by Recklinghausen (VIRCHOW'S *Archiv*, Bd. xxviii.), who, by giving them a supply of food (impregnating them with colouring matter), showed the migration of pus corpuscles in the meshes of a dead cornea. Stricker also (*Wiener Sitzungsberichte*, 1864) showed the movement of masses of cells in the construction of the body of the embryo, to form the rudiments of the organs. And Cohnheim, more recently (VIRCHOW'S *Archiv*, Bd. xl.) has shown that colourless corpuscles can leave the vessels, and migrate, and that thus there can be a transplantation of cells from one organ to another.

We have not space to detail the result of external stimuli upon the movement of cells, but will state that moderate exaltation of temperature increases them, and that they are variously influenced by mechanical influences, electrical stimuli, nervous excitation, and chemical stimuli.

The statement of Hermann, and also of Beale, that the spherical condition assumed by cells in water or other dilute medium, is a state of rest, is held by Stricker as plausible.

As to *metamorphosis of tissue*, indirect experiments justify us in admitting a metamorphosis of tissue in the cells. Thus muscular fibres are metamorphosed cells, and they consist essentially of contractile substance, the internal structure of which differs entirely from that of contractile protoplasm.

With regard to *structure of cells*, Stricker is inclined to admit that two functionally different substances are present, in which he considers that he is supported by the circumstance, that he has learned to recognize two active conditions, one in which the cell is dilated as after immersion in water, and one in which it is contracted. Though he says also that in the present state of microscopic science, we must admit that we are unable to see

[1] See a pamphlet by Montgomery, and a Review of the same in the number of this Journal for January, 1868, p. 198.

any differentiation of parts under the microscope, except in those cells or cell derivatives in which we are able to recognize definite peculiarities apparently associated with the performance of a specific function, illustrated in the optical peculiarities of the transversely-striated muscular fibres, and the apparent structure of ganglion cells.

In reference to the boundaries of the cell, we consider that the results of the observations of the author, confirmed as they are by his eminent German contemporaries and co-laborers, and the independent observations of Beale in England, are conclusive, and should settle the question. They are these :—

"We hold that the external limiting layers of protoplasm may undergo both physical and chemical changes, and there will then be produced a membrane that, compared with protoplasm, is of firm consistence. The recognition of a double contour line in the uninjured cell is indispensably necessary to prove the existence of an investing membrane."

This we have taught in this city for several years in lectures, and repeatedly called the attention of students to the effect of the *diffraction band* in giving the appearance of a limitary membrane when none is actually present, and are gratified to find the following quotation by Stricker from Brücke :—

"The difference between the density of the surrounding medium and the cell, even when no investing membrane is present, will cause the appearance of a boundary line; but it is only from the presence of a second contour that we are able to recognize the difference in density between the investing substance and the contained material. It is self-evident that the power of the instrument must not be pushed beyond its natural limits by the employment of strong oculars, as in that case a second contour line makes its appearance, not due to the structure of the cells, but consequent on defects in the optical apparatus we are using.

Again Stricker says :—

"If, moreover, a double contour line is observed after the action of reagents, no proof is afforded that a membrane existed during life."

To which the evidence of Kühne is also further adduced :—

"If an amœba were to surround itself by a broad hyaline border, not regularly defined internally, I should not be surprised if a reagent like acetic acid, which made this border suddenly shrink whilst under observation, were to cause the appearance of two wrinkled, closely approximated lines; and if I knew that this border was previously mutable, I should not believe the solid membrane, originating in the action of acetic acid, was previously present as an investment of the outer hyaline layer."

These facts and reasoning, doubtless, invalidate the results of Roberts and others, who claim to have developed a double membrane about the red corpuscle, as the result of the action of reagents like tannic acid.

In the section on the *nucleus of the cell*, the views already expressed are reiterated, that it cannot be regarded as indispensable in our idea of a typical cell. It is indeed known that when a nucleated cell divides, the division generally first occurs in the nucleus, but there are also non-nucleated cells that undergo division. On the whole, the function of the nucleus, as a special element in the act of propagation, becomes subordinate. Nor do we know much more respecting its physical peculiarities. The presence of an investing membrane is doubtful on the same grounds that lie against it in the case of the cell itself; especially is it inconsistent with the manifold changes of form the nucleus is known to undergo; though it is true that in many nuclei a double contour can be distinctly shown, as in many

ganglion cells, and composed of a substance of different nature from the contents. But it cannot be concluded from this, that the *nuclei* are *usually* vesicles. Our knowledge of the chemical characters of the nucleus is as limited.

The nucleus of the fecundated egg is said by Stricker most probably to disappear, while it is equally probable that the nucleus of the first segmentation mass is a new formation. But here our knowledge ceases. We cannot say that the latter, which is spherical and homogeneous, originates from the former, which is vesicular.

Our knowledge of the nature of the *nucleolus* is said to be still more limited. To this a special significance has been ascribed by Virchow in the act of propagation, but Leydig refuses to admit that it is of any importance. But Stricker says we cannot go as far as this, since the nucleoli in many cases develop into vesicles, on which still smaller nuclei may be distinguished. Here, too, it seems to us that the view of Beale, that the nucleolus is simply a new centre of germinal matter, is the only tenable one. It seems, however, to be imperfectly comprehended by the author.

Cell genesis.—Stricker admits three modes of cell formation, all from previously existing cells, 1st, by *fission*, in which the entire mass of protoplasm divides into two or more segments; 2d, by *gemmation*, in which a little elevation first projects from the cell—a bud—which is subsequently constricted at the base; 3d, by an *endogenous mode* in which the cells originate like embryos in the interior of the cell, and gradually increase in size.

The question, considered unsettled when the German edition of this volume was published, whether — since the migratory power of the white blood corpuscles has been ascertained—other cells besides these are capable of undergoing multiplication, has been settled by the more recent researches of Stricker,[1] which go to show that the division of pus-cells can be directly shown under the microscope, and that the proliferation of the cells of connective tissue by division, and of those of the epithelium by endogenous generation, are facts beyond dispute.

As to *forms of cells*, no general statement can be made as to that of amœboid cells, which are constantly changing their shape. Otherwise the divisions usually made are admitted.

The most significant statement in the section on the *union of cells*, as demonstrated by the silver solutions of Recklinghausen and others, is that the cement uniting them at their edges proceeds from a metamorphosis of the cell substance, and is therefore of the nature and should be included in the intercellular substances.

Classification of cells according to *function* is stated by the author to be the more usual one, though we should suppose this peculiar to Germany, since it is our impression that both in England and this country, a morphological basis, which is, however, much less philosophical, is used. The difficulties of the former will be great so long as the functions of certain cells are undetermined.

The *formative activity* of cells is seen in the fact that every organized portion of the animal body which is not a cell, must originate in or from cells.

The changes of cells in death are stated as difficult of determination in many instances, and, indeed, often impossible. The absence of amœboid movement is not necessarily associated with death. Nor do chemical rea-

[1] Stricker, Studien aus dem Institute für Experimentelle Pathologie, Wien, 1870.

gents enable us to arrive at a positive conclusion, unless their action is sufficiently slight to excite movements, but not to effect complete destruction. Again, it is often easy to determine that a particular cell is dead, as when it has been destroyed by powerful electrical currents, by a high temperature, or mechanical violence, all of which are often associated with remarkable alterations in form.

We have presented a somewhat extended abstract of this subject, because we deem it the most important chapter of the book. The remaining chapters, except, perhaps, the following on the connective tissues, also of great importance, will be more briefly considered.

In Chapter II., *The Connective Tissues* are treated by A. ROLLETT, Prof. of Physiology in Graz. The chapter is not quite as pleasant reading as the first, but exhibits an enormous amount of research and great familiarity with the comparative histology of the connective tissues, which are made to include *connective tissue proper*, *cartilage*, *bone*, the tissue of the *cornea*, and *dentine*—the usual classification.

Developed from the middle germinal layer, in which originate, also, blood and muscle, the connective tissues have this circumstance in common, that they contain extensive and continuous layers of material (intercellular substance), which, when compared with the cellular elements (protoplasm), distributed throughout its substance, or the morphological elements of other tissues, always appear as a *more passive* substance, and participate but slightly in the processes of life. The connective tissues also often pass by substitution or genetic succession into one another, appearing therefore morphologically equivalent; so that in many instances certain organs or parts of organs belonging to animals nearly allied, are formed, sometimes of one and sometimes of another. A further reason for considering them together, is the fact that they have similar modes of development, and show, therefore, a homologous significance with regard to their microscopic elements.

The consideration of the tissue of the cornea and of dentine is deferred to that of organs of which they form a part.

An excellent historical account of the different connective tissue theories is first given, including the views of Schwann, Reichert, Henle, Virchow and Donders, Max Schultze, and Beale—those of Max Schultze and Beale being almost identical, though independently arrived at. The observations of Waller, Von Recklinghausen, Stricker, and Cohnheim, with regard to the migration of white-blood corpuscles, are to be borne in mind in discussing all questions bearing upon the connective-tissue substances.

Connective tissue proper is primarily considered, first with regard to its cell forms. Studied in the indifferent menstrua, as serum, aqueous humour, or serum containing iodine, and especially with the aid of a moist chamber, the connective tissues exhibit, as demonstrated by Von Recklinghausen, the migrations presenting the characteristic amœboid movements of white-blood corpuscles. "The derivation of the migrating cells of the connective tissue from the blood has been certainly demonstrated in some particular instances, and rendered highly probable for all." They may be most conveniently observed and differentiated from the other cells contained in the tissue, in the tail of the tadpole. In addition to these, different forms of granular cells are met with, of which the most common are the *coarsely granular elongated form*. There are also certain *small, finely granular forms* which exhibit slow changes of form. In all, single, large, oval nuclei are developed by acetic acid. They are best studied in the perfectly transparent membranes between the muscles of the thigh and leg in the

frog, in similar preparations from newts and salamanders, and the loose connective tissue on the surface of the muscles of warm-blooded animals, as the masseter of a recently killed guinea-pig or rabbit. We also see, in the same tissue, *small fusiform less granular* masses having delicate elongated nuclei, intercalated among the parallel fibres of the several fasciculi; similar but *more rounded and flattened cells*, and *stellate* cells. The latter *anastomose* in the adult in the more independent connective tissue formations.

Finally, we have the *pigment* cells of connective tissue, occurring however in limited spots, as in the eye in man, and in the higher vertebrata, but widely distributed in amphibia and fishes, where they are generally stellate. Movements have been observed in the latter by Brücke and Buchholtz.

The solutions recommended by Rollett for the study of these cells are those of chromic acid, particularly that of Müller, consisting of $2\frac{1}{2}$ parts of chromate of potash, 1 part of sulphate of soda, and 100 parts of distilled water.

The earliest stages of all forms of connective tissue are described as consisting of cells which, for the most part, lie closely compressed, as in the small-celled sarcoma. As the result of development, however, two principal varieties are differentiated, one including *connective tissue plexuses and trabeculæ*, derived from cells, and which do not yield gelatine on boiling; the second, including the *fibrillar connective tissue*, characterized by the presence of peculiar, invariably unbranched fibres, yielding gelatine on boiling.

We must, however, pass over much interesting matter concerning the appearance, peculiarities, and modes of study of the fibrillar element in the varieties of these two divisions, to the most important subject of the *development of connective tissue*, pausing only to state that to the elastic fibres of connective tissue are ascribed the properties usually given them—sharply defined, rendered prominent by acetic acid, or boiling, with remarkable tendency to branch and form networks. In addition to their resistance to dilute or strong acetic acid, they resist, also, for a very long period, at ordinary temperature, the action of potash and soda; the latter of which serves also admirably to demonstrate them. Concentrated sulphuric acid only dissolves them after its action has been continued many days. They are not converted into gelatine by boiling, or by digestion with acids at 104°, as is the case with the ordinary filament. They are not believed by Rollett to have an internal cavity. Nowhere do we find used the term pure *white fibrous tissue* so common in English and American histology, and so useful in distinguishing the proper waving fibrillar element from the cord-like elastic fibres we have just been describing.

Nor will we occupy space with the historical account of the different theories of *development* of connective tissue, abundantly given in the various manuals of physiology[1] and pathology,[2] and by Rollett perhaps too briefly referred to, but proceed at once to the author's views, which are in opposition to those of histologists generally. It is premised that the denser connective tissue, as the tendons, do not afford the most favourable ground for the study of development, but better are the thin laminæ of serous membranes, also used by Henle and Baur. The peritoneum of the embryo of man and other animals, preserved in Müller's fluid, is con-

[1] Carpenter, 7th ed., pp. 38, 39.
[2] Virchow, Cellular Pathology, Am. ed., pp. 70–73.

sidered exceptionally suitable by the author, and, as the result of such study, we have the following :—

"From these observations it follows that, in the foregoing cases *any development of the fibrils by the growth of cell-processes must* be regarded as questionable.

"The fibrils appear to be formed simultaneously for considerable portions of their length. The cells contained in the embryonic mass which is destined to form connective tissue, either all increase in the process of development to fusiform cells of considerable length, at the same time separating from one another in such a mode that at first a small, but subsequently a gradually increasing number of fibrils appear between them, as in the tendons, or that at first a transparent, interruptedly striated substance occurs in great quantity, in which the fibrils become apparent at a later period, as in the peritoneum. This is what, in brief, I believe every one may convince himself of.

"As regards the significance of the large quantity of homogeneous substance which undergoes fibrillation in the peritoneum, with coincident elongation of the cells, it is difficult to make any positive statement. It can only be said with certainty, that *the fibrils originate at the expense of a large continuous mass by a kind of transmutation.*

"The most probable view then is, that the homogeneous intermediate substance which appears at a certain stage of development in the peritoneal lamina, originates in a continuous metamorphosis, extending irregularly towards the central portion of this rapidly enlarging cell-substance. The lamina thus originating in the fusion of the metamorphosed cell-substance becomes secondarily perforated with smooth-edged foramina, whilst a continuous conversion into fibrils takes place."—*New Syd. Soc. Transl.*, pages 90, 91.

From all of which we gather that the fibrillar portion of connective tissue is the result of a fibrous transformation of a homogeneous intercellular substance, which gradually separates the fusiform cells which originally constituted the young connective tissue. So far as the production of the fibrous substance is concerned, there would seem to be little difference between these views and those of Virchow and Kölliker.

In the New Sydenham Society's translation by Mr. Power, immediately previous to the paragraph last quoted is interpolated in finer print, but without explanation or comment, the following :—

"Further investigation has shown the original interpretation of Schwann to be the correct one, and that the fibrillæ of connective tissue take their origin from elongated cells, either by the splitting up of the cell-body into fine fibrillæ, or by the body of the cell becoming drawn out into one long fibrilla."

As this appears in the text, it would appear to be a continuation of Rollett's own writing, and it is only the utter incompatibility of its meaning with what immediately precedes that excites the suspicion of the reader. The fact of its being in smaller type is no assistance, since it is constantly the practice of Stricker himself, in the two previous papers, to place portions in finer type, though seemingly without any good reason. Nor is there any explanation in the preface of Mr. Power's translation which would lead us to expect any such additions. On the other hand, he distinctly states that some material which he had accumulated as indicating the progress made during the past twelve months on the subjects discussed, was omitted, but will appear in a condensed form in the *Biennial Retrospect* to be published at the beginning of 1871.

We have always held that many cells are constantly seen in the microscopic study of fibrous tissues, and especially of abnormal fibrous growths, which strongly point to the correctness of the views originally advanced by Schwann as to the development of fibrous tissue, and we thank Mr. Power for adding anything which would throw information on the subject, but we

think it should not be an interpolation in the text without explanation. It would better have appeared as a foot-note.

We have also always considered that in the matter of development of connective tissue Virchow was peculiarly unfortunate, since he here entirely abandons the proposition which he takes so much trouble to prove, that the cell is the starting-point of all life-action, healthy or diseased, in admitting the formation of the fibrous intercellular substance by the fibrillation of a homogeneous matter deposited between the cells, and not originating in any way from the latter. We deem much more consistent with a philosophical evolution of the cell-doctrine, as well as equally consistent with observation, the view of Max Schultze and Beale, which makes the fibrous intercellular substance originate from the conversion and simultaneous fibrillation of the protoplasm of membraneless embryonic cells, the remains of which, after the formation of the fibrils, are represented by nuclei with a little unaltered protoplasm about them, constituting the so-called connective tissue corpuscles.

As regards the formation of the elastic fibres, Prof. Rollett is consistent with his view of the development of the intercellular fibrous substance, though, strictly speaking, he may be said not to commit himself, since he simply writes "the opinion is now generally held that there is an actual deposit in the form of fibres," with references to Henle, Reichert, and H. Müller. It has already been stated that he does not consider them tubular, and there seems but little doubt that the view of Virchow of this nature of the fibrils is losing supporters; and it is also stated by Rollett that the view of Donders, which was that accepted by Virchow, for the development of these elastic fibres, has been proved erroneous.

As to the development of *fat-cells in connective tissue*, the process is that usually described, the conversion of young granular cells of the connective tissue into adipose vesicles by the substitution of fat, which first appears as a few small, highly refracting drops, which usually collect to form a single fat-drop occupying the middle of the cell. It is stated that "we possess no information from direct observation of the relation in which the protoplasm of the cell and the contained oil stand to one another in regard to their nutrition," from which we gather that the author does not admit the direct conversion of the protoplasm into the oil, but that a supply of material is furnished from the blood for the simultaneous production of both; though we do not see how he thus accounts for the final total disappearance of the protoplasm, leaving nothing but the vesicle and its contained fat.

In the section on *cartilage* the usual division into true or hyaline and fibro-cartilage is made, though the author distinctly states that such a division is no longer admissible, since it has been shown that just as the former consists of cells imbedded in a transparent and apparently uniform matrix, the latter is composed of similar cells in a matrix traversed by fibres, which would appear to us the best reason for doing just what he has done—adopting the classification. At first it seemed there must be some error in the translation, but the words *ist übergegangen* have received a proper rendering.

The tissue is admirably described, and directions given as to methods of its study which must result in thorough familiarity with its structure.

The cellular elements of the two cartilages are identical, and the fibrous element of the fibro-cartilages presents the characters of elastic tissue, its identity with which is proved by its being continuous in many instances with the elastic plexus of investing connective tissue and with true elastic tissue.

There is no tissue of which the fully-formed characters are better known than the *osseous tissue*. Accordingly, we find no new facts in the description of the minute appearance of fully-formed bone. The subject of the *development of bone* is, however, still one of disputed ground, but is so difficult of consideration without the aid of illustrations that we must refer our readers, either to the original, or the excellent translation of Mr. Power. It is sufficient to say that three modes of development of bone are detailed—the intra-cartilaginous, the intra-membranous, and the periosteal; though in all these cases the osseous tissue originates in an essentially similar neoplastic formation, called *osteogenous substance*, the above-mentioned differences referring to where the bone develops, and the presence or absence of cartilage: so that the process of osteogenesis itself is essentially the same in all.

As to the *contents of the bone-cavities;* the medulla, which occupies the central cavity and large medullary spaces, is composed of delicate connective tissue traversed by vessels, and containing numerous fat-cells, to which the yellow colour is due. This fatty medulla is, however, in no way to be compared with the young medulla in bones in process of formation, representing a stage of development in another direction, and possesses no osteogenetic activity.

The red medulla or substance of the medullary spaces of the spongy substance, is composed of a mass of bloodvessels, a few fat vesicles, and a large number of granular cells which exhibit amœboid movements. In this medulla are found the large many nucleated cells or masses of protoplasm described by Robin, and called by him myeloplaxes. These are most abundant in the external layers of the medullary masses.

Chapter III., *On the General Characters of the Structures composing the Nervous System*, is by MAX SCHULTZE. There is no subject in histology which has needed authoritative revision more than this one of the nervous tissues. The old division of nerve substance into two kinds of matter has become so stereotyped, that it seems almost impossible for teachers to admit the recent discoveries on this subject.

Schultze makes three divisions of nerve matter: 1. The nerve fibres. 2. The peripheric terminal organs of the nerves. 3. The centric organs of the nerve fibres.

I. The nerve fibres are subdivided by the author into six forms, of which *three* are *non-medullated* and *three* are *medullated*. Of the *non-medullated* variety: are first, *primitive nerve fibrils*, presenting the simplest form. "These are very fine threads which lie on the extreme verge of microscopic mensuration, and are only rendered visible with powers effecting an apparent enlargement from 500 to 800 linear." They are moderately frequent in the central organs and in the neighbourhood of the peripheric termination. "In such fibres as these no internal structure can be detected by the microscope. Their nervous nature is, however, placed beyond the possibility of question by their connection with ganglionic cells, and by the evidence of their issuing from thicker nerve fibres." They can be isolated only in preparations that have been carefully hardened by solutions of chromic and perosmic acid, by treatment with which and solutions of salts of different strengths, they also undergo a process of partial imbibition, occasioning the appearance of varicose enlargements, which subsequently increase in size and number, until at length the fibre becomes unrecognizable.

Second, *fasciculi of primitive fibrils* or naked axis cylinders, are distinguished from the primitive fibrils by their greater thickness. They are also

delicate, transparent, perishable, and isolable only for short distances. Their diameter is various, usually a few micromillimetres. They present a more or less well-marked longitudinal striation, resulting from a differentiation of the substance of the fibre, and from the probable presence of an interfibrillar granular substance. Their fibrillar character is most distinctly visible at their origin from the large centric ganglion cells.

The third form is the *peripheric non-medullated nerve fibre*, consisting of fibres composed of a thicker or thinner bundles of primitive nerve fibrils, according to the kind of axis cylinder present, united by a nucleated sheath of Schwann. It is perhaps scarcely necessary to state that by the *nucleated sheath of Schwann*, is meant what is commonly spoken of in our text-books as the *tubular membrane*, being the external envelope of the nerve fibril, and composed of nucleated connective tissue. The term *white substance of Schwann*, used by us to indicate the oleo-albuminous substance immediately surrounding the axis cylinder, is not used by Schultze, the term *medulla* being alone used for this. All the branches of the olfactory nerve in the mucous membrane of the nose of all vertebrates are composed of such non-medullated nerve fibres. They frequently also occur in the sympathetic, the branches of which distributed to the intestines are often entirely composed of them. Here they were first observed by Remak, and were therefore called Remak's fibres. These are also the nerves which, with few exceptions, are found among the invertebrata. *Gray, gelatinous fibres* are also appropriate terms.

The *medullated nerve fibres* include first, *primitive fibrils* with a medullary sheath, and second, *fasciculi* of primitive fibrils with a medullary sheath, consisting therefore of two constituents, a cortex of medulla or medullary nerve substance, and an axis fibre or axis cylinder, which is either a primitive fibril or a bundle of fibrillæ.

The third form of medullated nerve fibre is that which in addition to the two elements of cylinder axis and medulla contains also a *nucleated medullary sheath of Schwann*, and such are the majority of the peripheric nerves, except perhaps those of the optic and acoustic apparatus. This is either a structureless membrane analogous to the sarcolemma of muscle, or consists of several layers of fibrillar nucleated connective tissue. If very thin, the appearance of the nerve is not altered since the highly refractive external border prevents the extremely thin feebly refracting sheath of Schwann from being seen. These medullated fibres are those which early present the characteristic regular moniliform enlargements and irregular varicosities when removed from their natural condition. The presence of the membrane of Schawnn, however, often prevents these phenomena of imbibition from presenting themselves promptly, and also by imparting a firmness and resistance enables long portions of the fibres to be isolated. So, too, the marked difference in appearance and consistence of the medullated fibres from the central and peripheric portions of the nervous system is due to the presence or absence of this sheath, which is often of measurable thickness.

The same highly refractive character of the medulla makes it difficult to distinguish the axis cylinder, though it is always easily isolated for at least a short distance in the medullated fibres of the central organs, when the sheath of Schwann is wanting.

From the foregoing it follows that, according to Max Schultze, the primitive fibril forms the elementary constituent of all nerve fibres, the variations being dependent upon the number of fibrils, and upon the absence or presence of the medullary sheath and the sheath of Schwann.

He is supported in these views by Waldeyer, Pflüger, and Remak, but is opposed by Kölliker and Stilling.

A point which has long been proved, but which some physiologists seem loth to admit, is the *division of nerve fibres*. Occurring most frequently in the medullated nerve fibre, and at its peripheric extremity, as in the olfactory nerve, the mode of division is usually dichotomous, though there may be three, four, or even twenty-five branches, and it affects all the constituents of the nerve fibre, though the division of the fibrillar axis cylinder probably consists only in a gradual process of isolation of the associated primitive fibrils. The medullary sheath is, however, continued over the branches at the point of division, and is finally lost at their extremities. Most interesting and remarkable is the fact that at the point of division, in consequence of a sudden diminution of the quantity of nerve medulla, an attenuation of the nerve fibre occurs, while beyond this point the medulla is again found in the ordinary proportion, the sheath of Schwann dividing precisely in the same manner. The sum of the sheaths of the resulting nerves is much in excess of the original sheath, showing an augmentation in thickness. The medulla sooner or later disappears, and coincidently or later the sheath of Schwann also; the axis cylinder continues to divide and the fine primitive fibrillæ, as demonstrated by Cohnheim and Hoyer, "ultimately project from the subepithelial tissue between the cells of the tessellated epithelial layer of the conjunctiva corneæ, and terminate by free extremities at the surface." (*New Syd. Soc. Transl.*, p. 164.)

II. *Of the Peripheric Terminal Organs.*—We have in this section much that is new and complicated, of which the following furnishes an instance: "The peripheric division into primitive fibrils appears to occur in all the nerves of special sense, but especially in those cases where perception of a great variety of impressions occurs within a very limited space. *Peculiar terminal organs* are found in such instances in connection with each fibre. * * * In the nasal mucous membrane, fusiform, easily alterable cells are found occupying interspaces between the palisade-like cells of the olfactory region. These possess a centric and a peripheric process, of which the former exactly resembles the primitive nerve fibrils of the olfactory nerves, whilst the latter either ends at the level of the free surface of the epithelial cells, as in man, mammals, and fishes, or extends beyond this surface in the form of a long stiff hair or of several finer hairs, analogous to cilia, but incapable of movement." These cells are named *olfactory cells* and the hairs *olfactory hairs* by Max Schultze. So in the tongue, as shown by Axel Key, Schwalbe, and Loven, are contained *gustatory* cells, in the auditory apparatus *auditory cells*, while the terminal nerves in the retina present quite peculiar and similar features. Finally, the *tactile corpuscles* of the skin are each continuous with one or more medullate nerve fibre that divide in their interior, though the precise mode of termination of the primitive fibrils in them is not known. Similar, at least, are the nerve hairs found on the surface of young fish and naked amphibia described by F. E. Schultze.

Again, the corpuscles of Vater or Pacini may be regarded as constituting terminal organs of the sensory nerves, in each of which a medullated nerve fibre enters, and terminates finally in the axis cylinder only, which ends in a little bulb, the medullary sheath disappearing, and the sheath of Schwann becoming continuous with the laminated sheaths of connective tissue covering the corpuscle. This terminal axis cylinder has been shown under high powers by Dr. Grandry, to be also fibrous, and the terminal bulb to consist of finely granular substance from which the diverging terminal fibrils

can be distinguished. Similar are the terminal nerve corpuscles of Krause in the conjunctiva which only differ from the Pacinian bodies in the absence of the thick laminated investment.

The mode of termination of nerves in the transversely striated muscles are similar—as shown by Kühne and Engelmann, the axis cylinder either penetrating the sarcolemma and branching out to form the so-called terminal nerve plate, or, as in the frog, break up into primitive fibrils in the interior of the contractile substance.

In the unstriped muscles, according to Frankenhäuser, there is a connection between the primitive nerve fibrils and the nucleoli of the fibre cells. So, also, according to Pflüger, in connection with the salivary gland cells, the nuclei or the cells themselves, constituting the terminal organ.

No allusion whatever is made to the observations of Beale with regard to the termination of nerves in the papillæ of the frog's tongue, which would confirm also the idea of universal terminal organs, nor to his observations on the distribution of nerves upon the sarcolemma of striped muscular fibres, which is denied by Kühne and Engelmann, and probably also by Max Schultze.

III. *On the Mode of Origin of the Nerve Fibres in the Nerve Centres.*—This is by *ganglion cells*, the majority of which, "and it may even be said all," possess processes easily torn off in the fresh state. *These processes are axis cylinders* continuous with the cell substance, and there may be one or many, though in man and other vertebrata, according to Schwalbe, there is only a single non-medullate process which runs towards the periphery, subsequently becoming the axis cylinder of the medullated nerve fibre. With regard to the beautiful bipolar cells and their connection with spiral fibres, first described by Beale, it is stated that it is still a subject of dispute, as is evident from the conflicting statements of J. Arnold, Courvoisier, Kölliker, and others. The writer of this review is, however, informed by a gentleman who has worked in the laboratory of Stricker, since these pages were written in the original, that the existence of these bipolar cells and their connection with spiral fibres so long denied in Germany, is now being generally admitted. According to Beale, it will be recollected these are the only kind of ganglion cell met with, and that where there is but a single pole he believes the other to have been broken off. Max Schultze, however, states that in the sympathetic of the frog uni-polar cells are also met.

No mention is made of the researches of Lockhart Clarke upon the spinal cord and brain, observations which would seem to call for more than a passing notice, which they even do not receive.

Chapter IV., *On the Tissue of the Organic Muscles*, is by J. Arnold, and is, though a short chapter, unquestionably the most satisfactory on this subject which has come under our notice. It includes the consideration of the fusiform contractile fibres, the connective tissue, with vessels and nerves, with the methods of investigation according to which they may be best studied. The nerves are both dark bordered and pale fibres, which form three separate plexuses. The principal or *fundamental plexus* is composed of *large nerves*, in which ganglion cells lie scattered, and these are often collected into microscopic ganglia. It lies in the connective tissue surrounding the muscle bands or membranes. The secondary or *intermediate* plexus is formed by *pale nerve fibres*, which form meshes of moderate size and of rhomboidal or elongated shape, and bodies resembling nerve cells or nuclei with distinct nucleoli, lie at the junction. This network lies imme-

diately upon or beneath the muscular laminæ, and embraces the muscular bundles. From the intermediate network fine fibres are given off, which pass in among the muscular fibres and produce the *intra-muscular* plexus. From the intra-muscular plexus dark and peculiarly stiff filaments proceed, which penetrate the interior of the fibres and extend towards the nucleus. It has been stated in connection with the last chapter, that Frankenhäuser claimed to have shown that these fine nerve fibres penetrate the muscular fibre and go to certain granules of the nucleus which he terms nucleoli. But Arnold says that he was unable to recognize the actual extremities of the nerve fibres in the granules (nucleoli) of the nucleus, but considers these rather as the *nodal points of the finest nerve plexus, lying in the interior of the nucleus.*

Chapter V., *On the Mode of Termination of Nerve Fibre in Muscle*, by W. KÜHNE, contains the well-known researches of this observer on the vertebrata and invertebrata. The results are thus summed up by the author :—

"In all transversely striated muscles the nerves terminate beneath the sarcolemma, the sheath of Schwann becoming continuous with the latter. Up to this point the axis cylinder is accompanied by the medullary sheath. The extremity of the axis cylinder always corresponds to a remarkably broad expansion, which constantly forms a flat branching mass. This terminal nerve plate sometimes presents the character of a membrane, and at others resembles a system of fibres. In the greater number of cases the plate rests upon a substratum of nuclei and finely granular protoplasm, whilst in others this material is absent, and the nerve plates possess the so-called terminal nerve bulbs. The extremity of the nerve never penetrates into the interior of the contractile cylinder, and, on the other hand, never entirely invests it. Short muscular fibres usually receive only one nerve, but long fibres have several.

"We may add, hypothetically, that the substratum represents the remains of a formative material important in the development of both the muscular and nervous tissue, and that a similar explanation may be offered of the nature of the terminal nerve-bulbs in respect to the nervous tissue."—*New Syd. Soc. Transl.*, pp. 227–8.

Readers are generally familiar with these views, since they have been widely copied in the journals, and have been the subject of a long controversy between the author and Dr. Beale, whom the former characterizes as an energetic inquirer, but thinks he may have been deceived by the confusion of fibres traversing the accessory structures belonging to the muscle. It will be recollected that Beale contends that the axis cylinder of motor nerves forms a network *on* the surface of the sarcolemma.

The Behaviour of Muscular Fibres when examined by Polarzied Light, by E. BRÜCKE, is the subject of Chapter VI. Although these studies are in an unusual direction of histological research, they are not without comparatively early precedent, the polarizing microscope having been applied to the examination of muscular fibre by Prof. C. Boeck, of Norway, as early as 1839, and with somewhat similar results.

The phenomena are best studied in the Hydrophilus piceus, or large water beetle, and as described by Prof. Brücke, are these :—

"If the field be darkened by crossing the planes of polarization of the Nicol's prisms, those fibres only disappear which lie parallel to the plane of polarization of one or other of the prisms; the rest, which cut those planes at various angles between 0° and 90°, appear of a gray colour upon a black ground, the most distinct being those which cut them at an angle of 45°. In those parts where the muscular fibres running parallel with one another are arranged in several layers, the colour assumes a whitish tint, passing into yellow. The tint varies with the thickness of the layers, precisely as the succession of colours in Newton's rings, from the centre towards the circumference."

These phenomena are equally apparent when the fibres are thoroughly impregnated with, and surrounded by, highly refracting fluids, as glycerine, turpentine, or Canada balsam, and are due to the fact that "muscle substance is *doubly refractile*, two systems of undulations propagating themselves according to different laws, and interfering the one with the other." And this was the explanation offered in 1839 by Prof. Boeck.

As the result of further observations, Brücke concludes that there is an optic axis corresponding with the longitudinal direction of the muscular fibres, and that the sarcous elements are *uniaxial* and *positive*. By these means it is also learned that the sarcous elements are not elementary and simple solid bodies, but groups of *smaller doubly refractile bodies*, which Brücke calls disdiaclasts, after the phrase employed by Bartholin, the discoverer of double refraction in calc spar.

Of Chapter VII., *The Heart*, by SCHWEIGGER-SEIDEL, we must pass over all that precedes the distribution of nerves to the heart, of which it is said by the author that little is at present known. The nerves are derived from the cardiac plexus. They are mainly non-medullated fibres, and enter into communication with ganglion-cells, as shown by R. Lee, in the *Philosophical Transactions*, Lond., 1849. These, in groups, lie on the external surface of the fasciculi of fibres, sometimes forming small detached ganglia, connected with the nerve by a peduncle. The relations are better studied in the interauricular septum of the heart of the frog. The greater number of these exhibit the structure of the ganglion-cells of the sympathetic, that is from one and the same pole (according to our author) there originates the spiral fibre of Arnold and Beale. There are, however, also, true bipolar-cells, and ganglion-cells characterized by an arrangement "in apposition" (Auerbach), according to which two pear-shaped cells in a common sheath lie with their bases opposed, while their nerve fibres forming the apices extend in opposite directions. These are said to be mistaken for bipolar cells. No spiral fibre is here present.

Unfortunately the precise nerve with which these ganglia are connected is not known. As stated, the cells correspond in their anatomy with those of the sympathetic, and Schweigger-Seidel has found this form of cell in sympathetic ganglia, Auerbach in the plexus myentericus, while Kölliker has also convinced himself from anatomical investigation that these ganglion-cells have no direct relation with the vagus. Others, however, contend that these ganglia are connected with the vagus, and among them Bidder has stated that the spiral fibres are fibres of the vagus passing to the ganglion-cells. Finally, the author has never succeeded in detecting ganglia in the substance of the human heart, apart from those situated in the course of the fibres.

From the above it will appear that the subject of the ganglion-cells is by no means settled. Nor is that of the distribution and termination of nerve fibres on a more definite basis. This much is certain. The fibres are delicate, non-medullated, and divide repeatedly, running in the connective tissue *between* the muscle-cells (which are here destitute of sarcolemma), in the form of delicate nucleated fibres. These are, however, so obscured by the nuclei of the capillaries and muscle, that no course is left but to isolate the nerves by dissolving out the network of muscle-cells. As to termination, we have the barren statement that according to Kölliker the pale nucleated fibres in the frog terminate in the same manner as the nerves of the voluntary muscles; while according to Krause, the double contoured nerve fibres of the cardiac muscle end in motor plates, and hence the pecu-

liar operation of the cardiac nerves receives no explanation from the mode in which they terminate.

The amount of information gained in reading the chapter, and particularly the latter portion of it, is not great, but perhaps the subject could not be better handled.

In Chapter VIII., *The Blood Vessels* are considered by C. J. EBERTH, Prof. of Pathological Anatomy in Zurich.

The author of this paper concludes his section on the arteries with the clause "no general statement can be given that is appropriate to the entire arterial system," as to the elements which it contains. It would have been more systematic, perhaps, to have introduced the subject with these words. At all events, its import must be distinctly remembered in receiving the statement that there are five coats to the arteries, which, passing from within outwards, are, 1. A tubular system formed of a single layer of flat cells, called the endothelium; 2. An intermediate layer of longitudinal fibres of connective tissue between the endothelium and the internal elastic coat; 3. The internal elastic coat; 4. The muscular coat; 5. External elastic coat and tunica adventitia.

The universal presence of the first of these throughout the entire vascular system is a matter of comparatively recent demonstration, but that it is an essential constituent throughout is now well determined. The *intermediate layer*, composed of a finely granular substance with delicate transverse and longitudinal fibrils, according to Eberth, is only present in the larger arteries, and is gradually lost towards the periphery. The *inner* elastic coat, fenestrated on the larger vessels, calls for no special allusion.

The appearance of the *muscular* coat, on the scattered transversely disposed fusiform muscle cells, indicates the transition from the capillary into an arterial tube. This coat is by no means purely muscular, but is in all instances composed of a basis substance of finely granular connective tissue, containing scattered cells, and traversed by a few fine elastic fibrils, in which lie a number of muscle cells more or less closely packed. The relative quantity of muscular element and intermediate substance varies—the former increasing as the calibre of the artery diminishes, and the latter as it increases. Dividing the muscular tunic into layers are also a series of lamellæ of elastic bands and fenestrated membranes. But the longitudinal muscles of the arteries are chiefly situated in the *tunica adventitia*, especially in its internal and middle layers, where they seldom form a continuous layer, but are united into fasciculi of greater or less strength. The remainder of the adventitia consists of decussating fasciculi of connective tissue, with networks of elastic fibres.

The *Veins*, possessing thinner walls, less elastic and muscular tissue, and generally a stronger adventitia, require no further allusion.

The finer *Capillaries* consist only of a tube composed of cells or of a cylindrical layer of protoplasm. As they became larger, however, a delicate layer of tunica adventitia is added, which is formed, according to Eberth and Iwanoff, of a delicate network of fine fibrils, composed of the processes of stellate cells lying directly upon the vascular wall; each of the cells consisting of a large elongated nucleus, invested by an extremely delicate layer of protoplasm. Iwanoff alleges, but Eberth denies, that the capillary sheath constitutes a lymph space. Numerous examinations by the former of the adventitia of the larger hyaloid vessels treated with nitrate of silver, and undertaken with a view of detecting cells in it, have led only to negative results. Eberth proposes to call this layer of cells the external *vascular epithelium*, or better, the *vascular perithelium*.

Of great interest at the present day are the so-called stomata of Cohnheim, or, as they are better termed by Eberth, *intermediate spaces*. These appear as small, irregularly-shaped, dark, and sharply defined spaces, after treatment with nitrate of silver, *within* as well as *between* the cells. They are present in the arteries and veins as well as the capillaries, and in different number in different individuals. Eberth says:—

"To enable us to understand the passage of blood corpuscles through the vascular walls, it is not requisite that coarse spaces or openings should exist, provided we may regard the vessel as composed, not of a stiff, but of a *soft material*, forming an elastic permeable membrane. If the openings were really coarse, colouring particles of large size would pass through the vascular walls in various regions. But this never occurs. We do indeed see that fine colouring particles escape through the vascular wall, but this does not occur easily with those possessing the diameter of the colourless blood corpuscles. These, on the other hand, by reason of their softness and elasticity, accommodate themselves to the fine invisible pores of the vascular membrane, and having traversed these, regain their original form.

"Their escape must not, however, be regarded as a simply passive process, like the filtration of a colloid substance, to which it was likened in the first instance by Hering, for it can be influenced in the most various modes by the contractility of the cells. Everything, in fact, which favours or checks their active mobility, influences their extravasation."

Important in this connection, also, are the observations of Stricker,[1] which go to show the contractile power of the capillaries, and as the result of which it is not improbable that it is by means of such contractions that the corpuscles are pressed into the capillary wall, and ultimately made to traverse them.

In connection with this subject we would call attention to the Report of Dr. Woodward, the title of which is included in the caption of this review, which amply confirms the foregoing facts. It presents itself as one of the first and most satisfactory results of a method of work which nearest approaches that pursued in the German laboratories—work independent of, and uninterrupted by the numerous engagements of active practice. There is required but a brief comparison of the drawings and photographs accompanying the report to confirm their accuracy, and to prove that with proper facilities, Americans are competent to attain such results in scientific research as shall constitute contributions of the highest value, with regard to the structures and processes of life. We congratulate Dr. Woodward on the results of these labours, and hope they may be the forrunners of others even more brilliant.

Chapter IX., *The Lymphatic System*, by PROF. F. v. RECKLINGHAUSEN, constitutes one of the most valuable contributions made to modern histology. It is also, with a single exception, the longest paper in the volume. It would therefore be folly to attempt a connected abstract in the limited space of a review on so extended a subject. It is sufficient to say that two sections are primarily made of the lymphatic system: The *first*, containing the fluid which, immediately after its escape from the blood-vessels, circulates around the several elements of the organs, *the interstitial serous spaces;* second, the system of the efferent canals, the *proper lymphatics*. The latter, constructed on the same type as the blood capillaries, are likewise provided with stomata which have a similar purpose, but additionally, according to Recklinghausen, they also facilitate absorption.

With regard to the origin of lymphatics, after presenting the views of

[1] Wiener Sitzungsberichte, Band li. und lii.

Virchow and Donders on the one hand, and Brücke and Ludwig on the other, the author presents his own, which is "that the masses of connective tissue, whether they form the exclusive structure of an organ or are intercalated between the proper morphological elements of some other tissue, are traversed by fine canals, the *serous canaliculi*, which are directly continuous with the lymphatic vessels. These canals in many organs form plexuses, so that portions of them appear to be branched in a stellate manner, exactly resembling the connective tissue corpuscles." They are not, however, provided with a special wall, are not therefore tubes, nor are they mere fissures between the specific components of the connective tissue, as contended by Brücke and Ludwig, but are the *interstices of the fibrous fasciculi and lamellæ of connective tissue cemented to one another by a tenacious, homogeneous, firm material, in which the serous canaliculi are buried.* Von Recklinghausen says, however, that the general acceptance of these conclusions requires further evidence.

With regard to the origin of the lymph corpuscles, we have the following:—

"The follicular substance of the lymphatic glands is probably to be regarded as the chief formative centre for the lymph cells. It would, however, be going too far to say that the lymph corpuscles proceed exclusively from the lymphatic glands. The very precise observations of Herbst and Teichmann show that cells are already contained in the lymph of man and mammals before it has traversed the lymphatic glands. In all probability such corpuscles proceed from the connective tissue in which the lymphatic capillaries are distributed, and in the form of contractile connective tissue corpuscles may easily have migrated from the serous canals into these capillaries."

As to the *arrival* of the lymph corpuscles in the peripheric plexuses of the lymphatics, it is the arrival of the vagrant, colourless corpuscles of the blood, and they are consequently formed in those organs to which we refer the origin of the latter—the spleen and lymphatic glands; though the latter, in the circuit of the blood through them, would certainly furnish cells similar to those brought to them in the afferent vessels, which would also pass out by the efferent.

We commend to all medical men the careful perusal of this chapter, which we regret to leave so summarily.

Chapters X., XI., and XII., *The Spleen, the Thymus Gland, and Thyroid Gland,* are by WILHELM MÜLLER, E KLEIN, and E. VERSON. On account of our limited space we pass them over, but pause to consider briefly Chapter XIII., *On the Blood,* which, if not more important, is at least relatively *terra cognita.* It is by ALEXANDER ROLLETT the author of the paper on the connective tissues. Even here we must restrict ourselves to the consideration of such points as are of the greatest interest, or of a disputed nature.

Passing over, therefore, the description of the red corpuscles, except to demur at granular shading of illustrations where it is desired to give an idea of *smoothness,* we would direct attention to the fact here recorded as an observation of Max Schultze's, but one which many observers have undoubtedly noticed, and to which others, especially Dr. Beale, have publicly called attention—the occurrence of "*minute bodies, differing from the ordinary corpuscles in their spheroidal form and in some other peculiarities,* together with transitional forms between them and the ordinary corpuscles;" also to the statement some pages forward (415) that, besides the *white* corpuscles, there are *irregularly formed masses* of colourless globules which Max Schultze regards as *fragments* of cell substance. This is an

equally common observation, and, we may add, similarly interpreted. Now, these fragments of red and white corpuscles are undoubtedly the bodies which have in many instances been mistaken for *vegetable* spores, and so named—a conclusion which we consider as unfortunately hasty as ill-founded, since it has misled many who have lost and will lose much valuable time in setting themselves aright. The view here expressed, we believe, has further confirmation in the fact that blood corpuscles, as also stated by Rollett, p. 387, notwithstanding their great extensibility, may be broken up by mechanical means, as by pressure of a glass cover, which, after the lapse of a few seconds, is allowed to rise and is then suddenly again pressed down.

Under alterations of form of red blood corpuscles we find detailed, the results of evaporation and cooling of the blood, the action of mechanical agents, as pressure, and of viscid solutions which subsequently become hard; the action of electrical discharges and induction currents, increased temperature, alternate freezing and thawing, the addition of water, and salts, sugar, alkalies, acids, urea, neutral solutions of carmine and aniline, gases (watery vapour, carbonic acid, ether, chloroform, bisulphide of carbon and alcohol)—all of which are of more or less importance, but must be sought in the text.

The subject of the *structure of the red blood* corpuscle is of sufficient interest and importance to detain us briefly. In the first place, it need hardly be stated that it is described as containing no nucleus. Again, according to Rollett, *it is not vesicular.*

In a notice of "The Microscope in Clinical Medicine," by Dr. Beale, published in this Journal for July, 1867, we summed up the reasons, as they appeared to us, for and against the presence of a special cell-wall. It may not be improper to add a few additional reasons, pro and con, not there included, of which one or two, at least, are more recent.

Thus, *against* the existence of a cell-wall :—

1. According to Rollett, a vesicle filled with fluid, the parietes of which are yielding, and which again floats freely in another liquid, might be conceived to assume almost any form rather than that of a body with two concave surfaces, as in mammals, or with two convex surfaces surrounded by a circular or elliptical zone of a certain thickness, as in birds, amphibia, and fishes.

2. Again, according to the same author, experiments performed on the red corpuscles by all the various methods of mechanical agency, in no instance produce a form which can undisputably be regarded as the torn and empty investing membrane, and the occurrence of which is in no other way capable of being explained.

3. Nor do induction currents, electrical discharges, or freezing induce in the corpuscle, a condition directly proving the presence of a membrane.

4. The escape of the nucleus, 5, the coalescence of the coloured spheroids, 6, the physical remains after the discharge of the colouring matter, are also adduced against the vesicular nature, the former two of which, at least, and the latter in a modified degree, were included in the reasons in the notice alluded to.

In *favour* of the vesicular nature of the red blood disk, we have met the following: 1. Dr. Horsford, in the *New York Medical Journal* for January, 1869, has adduced, first, the *cusp shape*, often assumed by the red corpuscle when floating in serum. 2. Dr J. G. Richardson, of Philadelphia, in the proceedings of the American Medical Association for 1870, in addi-

tion to the already quoted action of water on the blood disk, presents the following: First, that when a mass of red corpuscles, such as occurs in dried blood clot, is washed with pure water, so as to remove all the hæmocrystallin, the outlines of the red blood disk may be readily detected on examination with a high power. Second, the appearance produced in the red blood corpuscle of the menobranchus by crystallization of its hæmatoglobulin—"being precisely that which would be produced by sticks of similar shape within an ordinary bladder partly filled with fluid."

Dr. Richardson says he has "several times seen a single crystal, as if increased in length, thrust out the ends of the oval corpuscle until the conjugate diameter of the cell became one-third greater, while its transverse dimension diminished to less than half its original magnitude, the nucleus being compressed closely against the side of the prism." Dr. R. also adduces the unfolding by pressure of the *crenæ* of a crenated corpuscle, as a reason for its vesicular nature. He says, also, that he has *twice* succeeded in cutting a corpuscle in two, with sharpened needles, on the stage of the microscope, as the result of which the coloured contents were immediately evacuated and disappeared, while the cell-wall immediately "shrank together, and became twisted upon itself and around the nucleus into a perfectly hyaline particle." Such a section seems to us most difficult, and Dr. R. says with regard to it that the observations were too few in number to be accepted as conclusive.

These reasons, although not without weight, are, unfortunately, like many others, both for and against the vesicular nature of the red corpuscle: they do not *necessarily* imply the condition with which they seem most compatible. All of the phenomena can be accounted for on the supposition that the corpuscle is possessed of a hardened exterior, which is indeed practically a cell-wall, and is capable of producing all the phenomena of one. We have always thought that the advocates of the vesicular structure attempted to prove too much in attempting to show a separate and distinct cell-wall which would peel off like the skin of a grape, while at the same time they have that which *functionally* is a cell-wall. We are ourselves satisfied of the non-vesicular nature, though we admit also that the reasons usually assigned for this belief are by no means always sufficient. In our belief, the best evidence in favour of the non-vesicular nature is—first, that a red blood-disk can be broken up into a number of small corpuscles which exhibit the same limitary characteristics. Yet it will certainly not be contended that each one of these little particles assumes a cell-wall immediately on the disintegration of the parent mass. Second, the absence of the double contour which should always present itself when a separable and distinct membrane is present. Every body sufficiently small to be examined by transmitted light has a *limit* or *edge* about it which more or less resembles a cell-wall, and this will continue so long as there is any difference between the refractive indices of the substance examined and that in which it floats; and so soon as these are made to coincide, the substance examined is no longer visible except it be coloured. We doubt, however, whether either side of the question is capable of proof which will satisfy its opponents, though the number who advocate the vesicular nature is certainly diminishing.

It should also be stated that, in the opinion of Rollett the author of this paper in Stricker's Handbuch, the results of the various inquiries which have been made with a view to determining the question favour his own view, "that a stroma or matrix enters into the structure of the coloured elastic

extensible substance of the red blood corpuscles, which exhibits so remarkable a similarity in all animals, and that to this the form and the peculiar physical characters of the corpuscles are due. Hence the conclusion, that, however complicated the chemical constitution of the substance of the blood corpuscles may be, yet, by the action of a series of agents, the colouring-matter can be separated from the stroma without causing the latter to lose its essential characters." We admit that the phenomena can be very generally accounted for on this supposition, but contend also that if a stroma constitutes the basis-substance of the red corpuscle, we should by high powers, at least, be able to see something of its structure.

With regard to the white blood corpuscle, we are not aware that any contend for its vesicular composition. The amœboid movement alone would be incompatible with this.

To one other subject only, in connection with this subject, can we allude, and that is the relative number of white blood corpuscles to red. On the authority of Welcker it is stated that there is one white corpuscle to 335 red, and on that of Moleschott one to 357. Boys have one colourless to 226 coloured; men, one to 346; old men, one to 381. Girls, one to 389; young women who are menstruating, one to 247; the same women when not menstruating, one to 405; pregnant women, one to 281. The observation with regard to menstruating women is interesting; the relative number during menstruation (1 to 247) is greater than in its absence (1 to 405), showing that the red corpuscles escape more abundantly than the white. This is contrary to the ordinary phenomena of diapedesis, in which the white corpuscle escapes more abundantly, though it is entirely consistent with the proper notion of that conservation which makes the red corpuscles originate in a transformation of the white.

Few recent studies have attracted more attention than those of E. F. W. Pflüger on the *salivary glands*. Chapter XIV., devoted to their consideration, has therefore been appropriately committed to this author.

The first circumstance which strikes us in this chapter is the deficiency of diagrams to illustrate the minute anatomy of the glands. Few subjects require more copious illustration than descriptions of minute structure, and the salivary glands, in the refined description they here receive, require it particularly.

A hyaline basement-membrane is contended for in the fine tubuli (salivary capillaries), which are considered analogous to the tubuli surrounded by the cells of the liver. The salivary cells of the alveoli are believed to be invested by a membrane, the protoplasm tenacious, finely granular, and frequently striated, and their size 0.014 millimetre in diameter. A single pale eccentric nucleus lies immediately beneath the membrana propria, of which the size is .006[1] millimetre. Of these the most remarkable peculiarity is that, when fresh, they give off an extremely delicate fibril, which often penetrates that surface of the salivary cell which is in contact with the membrana propria. The existence of this is corroborated by C. O. Weber and Boll, but denied by Kölliker and Heidenhain. This nuclear process is apparently hollow, since it often discharges a large quantity of tenacious material which clearly proceeds from it.

If the excretory ducts, lined with columnar epithelium, are traced peripherally, they become smaller as they subdivide, while their walls become thicker, not from the addition of any connective tissue, but in consequence of the elongation of the columnar epithelial cells, which, however, always

[1] Erroneously printed in the Sydenham translation 0.306 mm.

exist in a single layer. Besides this, the wider ducts exhibit more or less strongly-marked outgrowths lined with the same epithelium. If the ducts are traced further, fine passages are finally met with, .01 mm. in diameter, lined with the same epithelium as the larger, and terminating in blind extremities. These are the secretory tubules—*i. e.*, the capillaries of the salivary ducts having the same tenuity as the biliary capillaries, and leading to the alveoli. Not unfrequently, also, these form loops, or bend suddenly. Traced outward, the diameter of the cavity of these tubes is increased, essentially through the increased diameter of the lumen.

"The most remarkable feature of these cylindrical epithelial cells [lining the fine ducts] is presented by the surface turned from the canal, and which is immediately in contact with the membrana propria. From this spring a large number of extremely fine varicose hairs, quite a bunch or pencil of such hairs proceeding from each cell. The surface of the tube composed of these cylindrical cells, always easily capable of detachment from the membrana propria, appears, on account of the equality in length of the several hairs, like a thick brush. These extraordinarily fine fibrils may be observed in any of the fluids in which the fresh gland can be properly examined."—*New Syd. Soc. Transl.*, p. 431.

"Besides the salivary tubes, other tubes are found in the salivary glands, varying considerably in diameter, and lined by a small description of tessellated epithelium, that generally diminishes with the bore of the tube. *These may be injected through the ordinary excretory ducts, as well as through the salivary tubes*, and ultimately form, by their ramifications, passages which have only a diameter of 0.007 mm. or less, and are lined by a very small-celled pavement epithelium. These passages constitute, without doubt, excretory ducts proceeding from the alveoli, and form a stage in that developmental metamorphosis of the gland which exists even in the adult."—*New Syd. Soc. Transl.*, p. 432.

The exact relation of these last-described tubes and the salivary tubes and alveoli before described, we are unable to comprehend. Are the ducts, though of less diameter, outward prolongations of the salivary tubes, themselves the immediate outlets of the alveoli, although also the seat of secretion? We suspect the author himself is not altogether clear on this point. Whence the paragraph :—

"Whether the salivary tubes, which are continuous with these excretory ducts lined by pavement epithelium, communicate with the alveoli, and in what way this communication, if present, is effected, demands further investigation. I know for a fact that a mosaic of salivary cells may lie in immediate juxtaposition to columnar epithelium; *but it is very rare for the canal of a salivary tube to be directly continuous with a canal which is lined with salivary cells.* I am of opinion that the communication between the salivary tubes and the alveoli is effected by means of very fine passages (salivary capillaries)." (p. 432.)

There is not a little ambiguity in these passages. An illustrative drawing, even if merely schematic, might materially aid us.

Most important and interesting, since it is the subject of dispute, is the *minute distribution of nerves in the salivary gland.*

The nerve tissue of the salivary glands consists of ganglion cells and fibres, and of the latter there are both medullated in greater number, and pale nerve fibres. Of the latter also are three kinds: *a*, fasciculi of primitive fibrils, with a sheath of connective tissue containing nuclei; *b*, gelatinous fibres lying in a sheath of nucleated connective tissue, and probably composed of fasciculi of extremely fine varicose fibrillæ, producing the appearance of bands of finely granular protoplasm; *c*, bundles of tougher, more highly refractile, very fine fibres (.0005 mm.), also possessing a tube of connective tissue containing oval nuclei.

The medullated nerve fibres present some peculiarities, of which the most marked is the abundant division rapidly increasing towards the periphery. Medullated nerve fibres penetrate the membrana propria of the salivary tubes and split up their axis cylinders, the subdivisions of which wind around the outside of the epithelial cells to form a sub-epithelial plexus. These fibrils are accompanied for some distance by nerve medulla, as proved by the blackening of the osmic acid preparations around the termination of the thicker primitive fibres. These branch finally into the finest possible varicose fibrillæ, which have precisely the *same characters as the fibrils* (before described) *which emerge and join them from the columnar epithelial cells.*

"It is frequently observable that the last ramifications of the axis cylinder are continuous with these fibrils; and that the columnar cells thus represent the continuations of the finer and finest medullated nerves with the sub-epithelial plexus, is frequently capable of direct proof." (p. 437.)

In the *alveoli* two kinds of nerve terminations are perceived. In the *first*, the medullated primitive fibre perforates the membrana propria of the alveolus, and enters directly into the superjacent salivary cell by unmeasurably fine fibrils, which proceed from the nerve and pass directly and without interruption into the fibrils of the protoplasm of the salivary cells, these fine fibrils being detected only by very high magnifying powers.

The *second* mode of termination is by multipolar nerve cells, generally smaller than the salivary cells, the nerves being continuous with these cells, which are again brought into connection with the epithelial cells by their polar elongations. This is in opposition to the view of Kölliker, Heidenhain, and Boll, who consider these polar cells connective tissue corpuscles.

Somewhat peculiar are also the views of Pflüger with regard to the *regeneration of the glandular epithelium.* Primarily he considers that both new salivary cells and alveoli develop from the salivary tubes which have been described; that the young salivary cells are formed in *the processes* of the columnar cells, *without the nucleus being in any way implicated.* From which necessarily results the following:—

"As on this ground I do not feel myself justified in attributing the origin of the new nuclei developing in the processes, to that [the nucleus] of the columnar cells, we must admit that we have before us a case of *free cell* formation, if under this term we understand that mode of cell increase in which the newly-developed nucleus originates independently in a cell, and is not a morphological element proceeding from a division of a previously existing nucleus."

Nor does this alone follow, but—

"When we see the axis cylinder and its fibrils to be directly continuous with the fibrils of the columnar cells, without any difference being perceptible between the axis cylinder and the fibrils of these cells, we may legitimately describe the nerve as extending to the point where it joins the substance of the body of the cell. That is the most natural explanation that can be given. This explanation, however, possesses the greatest significance in regard to the mode of development of the glandular epithelium, because it directly follows that the *young nuclei originate in the axis cylinders*, and that the *gland cells* which at a later period seem to constitute a thickening of the axis cylinder *bud forth, as it were, from the nerves.*"

These are startling views. Yet they are based upon seemingly accurate observations of the most accurate school of observers, and we are unwilling to judge them. We were unable to learn on a careful reading what are the nerve trunks thus distributed. Are they the branches of the sympathetic, or those of the chorda-tympani and auriculo-temporal? We

infer, however, that the filaments thus terminating are supposed to be derived from the sympathetic, from the fact that ganglion cells are described as accompanying the nervous cords for a considerable distance. Of course, if this is the case, and the observations of Pflüger are correct, the views of Bernard, which seem so conclusively proven, of the antagonistic action between the sympathetic distributed to the bloodvessels and the motor nerves distributed to the gland, must fall to the ground. That this point was not unthought of by Pflüger would appear from the following paragraph :—

"In some instances a single ganglion cell is found in the course of a fibre of Remak. It is remarkable that a large ganglion cell of this kind, having a diameter of .042 mm., may contain several nucleoli ; and, moreover, at the point of transition into the nerve fibre, may present a slight deposit of protoplasm, with several ganglionic nuclei ; and I desire particularly to direct the attention of observers to this singular form of ganglionic substance. The relations of the ganglion cells of the gland are also deserving of special investigation, which will certainly bear upon the physiological point of whether the *sympathetic is distributed exclusively to the bloodvessels, or whether it does not stand in intimate relation to the secreting cells.*"

The essential morphological constituents of saliva are correctly said to be in health none, though as accidental elements and as the result of irritation, we may find isolated cells and groups of epithelial cells with nuclei, *unquestionable medullated* nerve fibres, connective tissue, the so-called *salivary corpuscles*, that is, small, granular nucleated cells, presenting in some instances amœboid movements; whilst the fluid is rendered cloudy by small granules. These salivary corpuscles, according to Pflüger, are derived from the *excretory ducts*, and not from the gland.

Some pages are devoted to the Alteration of Structure in the Glands, caused by the performance of their function, the Stroma of the Salivary Gland, and, in conclusion, the method of investigation pursued.

The *fresh* tissues are preëminently necessary for successful study, and sections of these are examined in iodized serum, or the latter is used subsequent to maceration in chromic acid of $\frac{1}{4}$ to $\frac{1}{2}$ of one per cent.[1]

Chapter XV., *The Structure and Development of the Teeth*, is by W. WALDEYER. Of this tissue, as of bone, the fully formed structure is accurately determined, so that there is little additional in the descriptive portion, nor do we find anything in the subject of the development of teeth to justify further extending a review already much prolonged. The subject is well treated, and the paper is one to be consulted by all students of dental histology.

Chapter XVI., *The Intestinal Canal*, is the result of the joint authorship of E. KLEIN and E. VERSON.

The *Oral Cavity*, by Klein, contains as its most interesting organ the *Tongue*, of which the minute anatomy is in the main well determined. The termination of the nerves in the papillæ, with regard to which alone there is some dispute, is apparently not treated. The *Pharynx* and *Œsophagus* are considered by the same author, as is also the *Stomach*. The description of the stomach glands in no important degree differs from that met in the recent text-books on physiology. No allusion is made to the now well-determined transition of the walls of the alveoli in the cardia, into rudimental villi as we approach the pylorus, originally proven by the admirable injections of Dr. John Neill of this city, whose paper appeared

[1] Erroneously printed 25 to 50 per cent. in Powers' Translation.

in the *American Journal of Medical Sciences* for Jan. 1851. We believe that in no tissue excepting this, does the opaque injection exhibit peculiarities of vascular arrangement more satisfactorily than the transparent. Certain it is, that in opaque injections from the cardiac, middle, and pyloric portions of the stomach, we have satisfied ourselves of this transition, while transparent injections fail to show it. These subjects are sparingly illustrated.

The *Small Intestine* is by E. Verson, and is more attractively treated than the portions of the alimentary tract just named, but for the subject of greatest interest in this connection, the origin of the lacteals, we are referred to Chapter IX. on the Lymphatics.

The so-called *solitary glands*, which in their aggregation constitute the Peyer's patches, are spoken of as *lymph follicles*. Their lymphatic nature is considered as determined. They are roundish, pyriform or more *flask-shaped*, dipping into the submucous tissue with their rounded extremities, whilst their thinner ends form projections on the free surface of the mucous membrane, piercing the muscularis mucosæ, the fasciculi of which separate to permit their passage. With regard to their minute structure we have the following:—

"However delicate a section may be that is made through a follicle, only an irregular accumulation of cells can be recognized; but if these be removed by pencilling with a camel-hair brush, or still better, by agitation of the preparation in a test-tube half filled with water, a network or plexus of fibres comes into view, similar to, though somewhat closer than that presented generally by the mucous membrane of the small intestine. The follicles, consequently are composed of a plexus of fibres, and of cells (lymph-corpuscles) which fill the interspaces between them. But, just as the plexus of the mucous membrane presents histological differences under various circumstances, so may the framework of intestinal follicles differ, sometimes appearing as a tissue of anastomosing cells, the nuclei of which coincide with the thickened nodal points (child, rabbit), sometimes as a plexus of rigid hyaline trabeculæ (adult man, cat), and sometimes as a fibrous network (young dog)."

We cannot omit mention of the *muscularis mucosæ*. There is a layer of organic muscular fibres lying between the mucous membrane and the submucous tissue, discovered by Middeldorpf and Brücke, and extending from one end of the intestinal canal to the other, and from which processes are given off in various directions. Here, also, two laminæ of nearly equal thickness, of circular and longitudinal fibres, can be distinguished, though in some places they run into one another.

The *large intestine* and *rectum* are treated by the same author. These papers are illustrated by some admirable drawings, two of which longitudinal sections of the small and great intestine throughout all their coats, should be in the collection of every lecturer on anatomy and physiology. Another of the stomach completes a valuable series.

Chapter XVII., *On the Bloodvessels of the Alimentary Canal*, completes the first volume of the New Sydenham Society's translation, and includes to page 429 of Stricker's Part III. It is by C. Toldt. Appropriate drawings illustrate the arrangement of the bloodvessels in the mucous membrane of the oral cavity, the tongue, pharynx, and œsophagus, the saccular glands of the mouth and pharynx and tonsils, acinous glands of the alimentary canal, mucous coat of the stomach, muscular and mucous coat of the intestine, the solitary gland follicles, and Peyer's patches. To these the reader is referred. Description is unavailing without them.

A few words only about the volume as a whole, and we have done. We have no hesitation in declaring our belief that it is in every way the most useful work on Histology that has ever appeared from any press, and that the New Sydenham Society have perhaps never republished a work which will be more generally sought for, or which will contribute more largely to the popularity of its issues. Both original and translation are as free from defects as we could perhaps expect them to be. Had the original been a little more liberally illustrated in some of its parts, the text would have been clearer. We instance again the paper of Pflüger on the Salivary Glands. A more general indication of the degree of magnifying power would be useful for readers. The engravings in the Sydenham Society's Translation are not quite as good or as well printed as the original, and the frequent omission of lettering necessitates longer time in the interpretation of them. A few typographical omissions might be remedied by a list of errata. The most important of these is perhaps the reference to Cohnheim's paper on page 16, which is to Band XI. Virchow's Archiv, instead of Band XL. Elsewhere the same reference is correct. A few similar errors have been corrected in foot-notes as we progressed. But to avoid such errors as these is almost impossible. The translation is generally good, though occasionally ambiguous. Thus, the sentence at the bottom of page 144 and top of page 145, corresponding to s. 105, I Lieferung, might be made clearer, and the word *sichtlich* translated *apparently*, would seem to be better, *evidently* or literally *visibly*, in contrast with *wenigstens zum grössten*, at least in greatest part.

We mention these small points, not in a spirit of adverse criticism, but simply with a view to affording assistance in matters more likely to be overlooked by a translator absorbed with the primary object of his work, than by one reading for information. J. T.

ART. XIX.—*A Treatise on the Theory and Practice of Obstetrics.* By WM. H. BYFORD, A. M., M. D., Professor of Obstetrics and Diseases of Women and Children in the Chicago Medical College, etc. etc. 8vo. pp. 457. New York: Wm. Wood & Co., 1870.

FROM ancient history we learn that a certain Milo, after vanquishing all the athletes of his day, and after performing other marvellous feats of strength, finally attempted in his old age to crown all by rifting open a knotty tree. But the effort was too much for him; his strength gave out, and, his hands becoming inextricably pinched in the cleft, he miserably perished.

Let us apply the moral: Some years ago Prof. Byford wrote a capital work upon the "Diseases and Accidents incident to Women." His grammar, it is true, limped badly, and his periods were none of the most sonorous; but then, as a make-weight, the matter was excellent. It is about the most dog-eared and worst thumbed book in our library. Other valuable contributions to science have appeared from his pen which do him—if not a *learned* profession—credit. But, alas! flushed with these laurels, Prof. B. has once more entered the lists, and this time we fear has attempted the tree-feat.

We dislike to say it—for our prepossessions are in favour of Prof. B.—but he has written a book on a grave and important subject in too breathless

a hurry to respect the rights of the nine parts of speech, or the wants of the reader. So glaring are the faults; so rampant are the Americanisms, as our cousins across the water would call them; so abandoned the grammar; so *jejune* the matter, that we scarcely know where to begin our review or where to end it.

The preface states that—

"In the course of his teaching for many years the author has been convinced that a treatise much more concise than any of these [Hodge, Cazeaux, Meigs, etc.] could be made to contain all the practical information necessary to guide the student and 'busy practitioner' in the acquirement and exercise of this important branch of our art, and it is with the hope of accomplishing this desirable object that the present work has been undertaken. . . . It is hoped that while this work is concise, it will be found to be comprehensive and complete, and to contain in perspicuous detail all that is regarded as practically valuable at the present time."

If *conciseness* means to leave out verbs, drop articles, and ignore prepositions; if *comprehensiveness* and *completeness* imply prolixity on trifles, sterility on essentials, and the audacious adjudication of the moot questions of science, then the author has certainly achieved his end.

As a literary curiosity this book stands alone. Scarcely a page is free from gross verbal inaccuracies, or violations of the ordinary rules of syntax.

Not content with taking unwarrantable liberties with his own mother-tongue, he makes free with a dead language. Genius is proverbially independent, and *his* surmounts the difficulties of the declension, gender, agreement, and number of Latin parts of speech with no help from Zumpt or Scheller. By the simple device of an *i* final—why have not grammarians thought of this before?—he converts the singular number into the plural, as in "*ovi*," "*fœti*, and "*perinæi*." Throughout the book the words "*primipara* and *multipara* are used indifferently in the singular and plural number, sometimes as nouns, and sometimes as adjectives. At page 220 we have a "post-*partem* hemorrhage;" on page 380 "one of the line*a* semilunar*is*;" on page 287 we find a "delivery *per viâ naturalis* entirely impracticable," to which we cordially assent, for the liveliest fœtus could not possibly extricate itself from such a tangle as a noun in the ablative, qualified by an adjective in the nominative, and governed by a preposition requiring the accusative. At page 239 the difficulties of the fœtal circulation are still further embarrassed by a "ductus-arteriosis" and a "ductus-venosis.' His prescriptions must be models in their way, if we may judge from such specimens as pul. ergot; sul. ether; ant. et pot. tart.; aromatic sul. acid; persul. iron; bitart. potassa; and pil. hyd. Then again as there is but *one* Cazeaux known in obstetric literature, Prof. B., with a great show of accuracy, invariably writes the name in *his* singular number, viz., "Cazeau," without the x final.

But his crowning feats are performed in his own mother-tongue. At the risk of proving tiresome we will group together a *few* of them. At page 41, undoubtedly for meritorious services, he gives the accolade to the uterine cavity, and dubs it the "*corporal* cavity," At page 57, the walls of the cervix "*similate* the feel of membrane." Page 68 contains the following literary curiosity of a sentence without a verb, and this is by no means the only one:—

"At first entirely within the pelvis, with the bladder before and somewhat overlying it; the intestines on and around the fundus, and the ovaries and Fallopian tubes rather above the fundus during the first few weeks."

On page 159—

"The membranous *portion* of the ovum *is* not unfrequently the subject of diseases giving rise to abortion. *They are* occasionally so fragile as to rupture and discharge the liquor between the chorion and amnion. . . . . Among several cases I have seen was one in which the *discharge of what*, at the time, was considered to be fluid from between the chorion and amnion, beginning in the sixth and continuing with intervals of only a few days during the whole of the subsequent months of pregnancy."

Will Prof. B. kindly parse "*discharge*" in the above sentence? We quote the following from page 164:—

"Many cases of habitual abortion take place *something* in this way: the menses are regular for three, four, or even more months, and then *it fails* to appear at the usual time, but *comes* on in two weeks after time, or at [*sic*] next period *is* more copious and painful."

Abortion is treated of thus on pages 170 and 171: "We shall seldom succeed when the *premonitory* symptoms *do not antedate* the actual commencement. . . . Plumb. sub. *act.* mattico, and *other* mineral and vegetable *tonics* are used by many physicians to restrain the bleeding. . . . If the symptoms are such as to make it appear impossible to arrest the *process* [fœtus?] we must promote *its* expulsion as speedily as possible, as its presence is a source of danger to the mother." Page 178 rejoices in the following sentence, which is beyond our grammar as well as our comprehension: "Now what the existing circumstance which brings on the ungovernable tenesmus that will not be allayed until the body which has become a source of irritation and pain is extruded, is, like other ultimate facts, occult, and the proper subject for speculation, but of no practical importance." In such a costume "ultimate facts" will always be "occult."

We glean from page 240 that "the eyeballs had *bursted*," and that "there is a peculiar limpness and laxness in the joints and limbs of a recently dead fœtus that *is* enough in *itself* to distinguish it from one in a state of apnœa." When the bladder descends in front of the head he tells us, on page 292, "to *raise the extension up* and keep it in place until the head passes. And sometimes the forceps will be required after the bladder is evacuated, because the *viscera* cannot be elevated above the head, and the distress caused by pressure upon *it* mars the efficiency of the pains to such a degree *as to render them entirely inefficient.*"

We owe an apology for so many quotations, but Prof. B. performs so remarkable a feat on page 382—a feat, by the way, which "a man of the Pharisees, named Nicodemus," thought impracticable—that we really must transcribe it. "After the child is removed, ligating and cutting the cord should be left to an assistant, while the *operator*, with liberated hands, *may re-enter the uterus* to grasp the placenta." After so startling and unexampled an exploit we ought to let the curtain drop, but we crave space for just a few more sentences, some of which we defy any physician from the St. Lawrence to the Rio Grande to explain. On page 387 we read that "the persul. iron, applied on lint to the os uteri, causes the blood that comes in contact with it to coagulate, and thus acts as a means of retaining the blood itself as a plug in the os uteri until *that is nearer and in the mouths of the vessels to coagulate.*" To distinguish a breech from a vertex presentation he says, on page 208: "The breech sinks well into the pelvis, ordinarily, before the beginning of labour, when it is the presenting part, and generally fills the pelvis in all directions; yet if we press the finger on any four opposite points—at the sides of the pelvis

—of the presenting part, in some one or more of the points we will find soft tissues, instead of all bony as in the vertex; *for this experiment with the vertex will show four points of solid bone when the knees present.*" We call upon Prof. B. to give us the exact length of his index-finger in *feet*. We demand this as a solemn duty he owes to the profession. We have often violated the tenth commandment when a brother practitioner's fore-finger—we have one now in our mind's eye—exceeded ours in length by half an inch; but our wildest aspirations never reached a finger capable of touching "four points" on the vertex "when the knees present." At page 266 the author attributes the frequency of tender nipples to the custom of "covering the breasts with impermeable or large quantities of *goods*." *Goods!* what *goods?* We protest against the ambiguity of this expression. Does cotton, in the raw state, ever go under the name of *goods?*

We might multiply instances of such sins of omission and of commission—his trade-marks we might call them—but it is high time to glance at the matter provided by Prof. B. for "the student and busy practitioner."

The text from the beginning to the end flows on in one monotonous stream, unbroken by a single chapter or section; but the words "Theory and Practice of Obstetrics" are placed at the top of each page, so that, including the title-page and the gilt lettering on the back, the reader is needlessly reminded exactly four hundred and fifty times that he is reading neither a collection of essays from the *Spectator* nor a work by Lindley Murray. The first seventy pages are taken up with the theory of generation, and the minute anatomy of the womb, ovaries, placenta, and ovum; but they are written so hastily, and look so formidable without any pauses for reflection, that we feel assured no "busy practitioner" will waste much time over them; much less the "student," unless he has taken Prof. B.'s ticket.

But, apart from the slipshod style and the careless make-up of this volume, the information imparted is not the most instructive. Twice does the author inform us that in eclampsia "the blood is surcharged with urea and its elements; that this substance or the carb. ammonia, into which Frerichs thinks it changed, in circulating through the nervous centres, renders them more excitable," &c. Surely Prof. Byford, who writes in 1870, ought to know that positive experimentation has refuted this theory, and that Frerichs himself has long since abandoned it as untenable. At page 66 the author states dogmatically that "the nerves undergo just as great hypertrophy as the rest of the uterine tissues, being visible in large bands beneath the peritoneal membrane, running in various directions over the anterior and posterior surfaces particularly." We humbly protest against this judicial finding on a question which still remains vexed by the best anatomists and physiologists of the day. Such men as Lobstein, Osiander, Longet, John Hunter, and Snow Beck have either denied that the womb possesses any nerves whatever, or else hold that they do not increase in size during pregnancy. On the contrary, Wm. Hunter, Remak, Kölliker, and Lee as positively assert that the uterine nerves become hypertrophied during gestation, and subsequently undergo atrophy. Whilst Robin, Heschel, and the best modern microscopists contend that it is the neurilemma alone of the nerves which enlarges. Under these circumstances it seems hardly fair that the "busy practitioner" and "student" should be misinformed as to the extent of our knowledge upon this point.

Again, on page 127, we find the confident assertion that "the course of

phthisis pulmonalis is often very decidedly influenced by the supervention of pregnancy. All the symptoms are generally ameliorated to a degree that is sometimes surprising, and occasionally the advance of tissual destruction in the lungs seems to be stayed." We grant that this opinion is also shared, to a limited extent, by some eminent physicians; but since such men as Virchow,[1] Grisolle,[2] and Dubreuille[3] take high ground against it, and are able to marshal a formidable array of facts and figures in the encounter, we think Prof. B. might have used less positiveness in the assertion.

Having, very unfortunately for the reader, invented a pelvimeter—and a very good one it is—he devotes *eleven* pages and *ten* wood-cuts to this comparatively trivial instrument; whilst the important consideration of version in contracted pelves—a subject which is at present agitating the obstetric world, and upon which more light is wanted—is exhausted in *one* short page, including the space taken up by two borrowed wood-cuts, which he condescends neither to explain nor to give credit for. Again, a friend has invented a barbarous instrument, called "Evans's obstetric extractor," which, in these forceps days, is simply a relic of the dark ages. On the elucidation of this wretched fillet he squanders three mortal pages and three immortal wood-cuts; in one of which, by the way, the hand of the operator is represented as hypertrophied, either by disease or by false perspective, to double the size of the lassoed fœtal head. On the other hand *five short sentences* are doled out to the consideration of the subject of spontaneous evolution and of "the other method of expulsion," to which he vouchsafes no name for the benefit of the "busy practitioner and student." By adhering to the obsolete terms of Denman, he calls that process a "spontaneous evolution" which is recognized by all modern authorities as "spontaneous version;" and *his* "other method of expulsion" is *their* "spontaneous evolution." Barnes demands three chapters and twenty-eight pages to elucidate this important as well as difficult subject, Denman and Douglas wrote essays upon it, but it remained for Prof. B. to cut the Gordian knot by one trenchant sentence. "I think," he says, "the term spontaneous evolution is just as applicable to this as *the other method of expulsion*, the head being the last part extruded from the organs." We think, however, that if the author would put himself in the place of the fœtus—a feat he is quite equal to, from his own showing—he would speedily see a distinction and a difference between a delivery of the breech by the process of substitution, and its delivery by being unfolded from a doubled-up body.

At page 96 the author has managed to convey a meaning precisely opposite to the one intended. He wishes to say that occasionally in the newly married, who are not yet pregnant, instances occur of complete cessation of the menses for several months. To do this, he uses the following language: "But occasionally we meet with instances, especially in the newly married, where the menses do not make their appearance for several months, *without the existence of pregnancy*, and the person enjoying the best of health." The same form of blunder occurs on page 130, in the following sentence: "In using particular remedies, or classes of remedies, during pregnancy, we may be governed by the general consideration of avoiding the production of abortion *by exciting the uterus to action, inducing congestion in it, or employing means that act deleteriously on the fœtus;* and to get a more definite understanding in the

[1] Virchow's Archiv, 1870. [2] Archives Générales, Jan. 1850, p. 41.
[3] Bullet. de l'Académie de Méd., vol. xviii. p. 14.

matter, I propose to allude in a very brief manner to some of the more prominent *agencies* likely to be used in the disease [*sic*] of pregnant women."

At page 98 we find this most remarkable experience in the vomiting of pregnancy:—

"*With* some practitioners there is believed to be some indefinable condition of the nervous centres, not unlike sea-sickness, that requires nerve stimulants, as brandy, ammonia, strong coffee, and other such remedies; and these, too, have sometimes a very happy effect. We do not know the essential condition giving origin to this many times distressing symptom. All we know is that it is one of the sympathetic effects of the uterus in pregnancy."

Surely the author does not believe that the vomiting of pregnancy is a contagious disease, and liable to be contracted by the attending physician. If so, his experience is totally at variance with that of the profession at large. We have, it is true, known quite a number of medical men who, for "some indefinable condition of the nervous centres," have found it necessary to take "nerve stimulants, as brandy, strong coffee," and even whiskey and lager beer, but we never supposed that this habit was "one of the sympathetic effects of the uterus." We were in hopes that the preposition "*with*" in the above sentence was a misprint for the preposition *by*, but upon reaching page 444 we find a hint thrown out that even puerperal albuminuria may also be caught by the attending physician, who, in the reported case, was probably a female practitioner of some note. "A case recently occurred in the practice of my colleague, Prof. Roler, in *whom* œdema and albuminuria persisted for several weeks before confinement, that was carried safely through, mainly, I think, by the colchicum."

The whole subject of the forceps, as treated by the author, is far behind the age, and reads like an extract from an English work on obstetrics. The straight forceps is a make-shift, which even British practitioners are beginning to banish from their bags. The only advantages claimed for it are the doubtful ones of favouring an anterior rotation in occipito-posterior positions, and of being applied with greater facility with the patient on her side. But Prof. B. cannot offer even this latter excuse, for he places the woman in the dorsal decubitus, and yet says that "the short straight forceps will answer all purposes," excepting when a head is arrested at the brim, or requires powerful compression. The short straight forceps is his favourite instrument; and we wonder not at this preference, after reading the following inaccurate reasoning in his characteristic style.

"It will be understood by an examination of the curved instrument that the curve is intended to adapt itself to the direction of the axes of its pelvic cavity, and that when we introduce it crosswise or diagonally, with reference to the pelvic curves, that there is much danger of touching the sides of the pelvis with the ends of the blades, and bringing them in contact with the ramus of the ischium at the outlet, and thus dangerously contuse the soft parts at these points. I think, indeed, that there is very much more damage done to the soft parts of the mother by making this diagonal application of the curved forceps than there would be if the rule was to use them with reference to the pelvic curves entirely."

The blades of the forceps are constructed for application to the sides of the child's head, and, when applied thereon, are lost upon them, and can then do the least harm both to the mother and to the child. If, however, they are applied "with reference to the pelvic curves entirely," and diagonally to the child's head; then, not only the tip of one blade and the

edges of both may project and injure the maternal tissues, but the child may suffer as well from the unequal pressure of their edges which do not then adapt themselves to the parts seized, as from the slipping of the blades, which takes place more or less whenever they are not applied to the sides of the child's head. An eye may be squeezed out, an ear torn off, the cheek and scalp seriously injured, or the child's life may be quickly destroyed by the pressure of the occipital blade upon the medulla oblongata. With commendable frankness the author himself confesses that in one instance he lost "a patient from hemorrhage resulting from a wound in the scalp near the frontal protuberance," produced by the very "diagonal" application of the forceps which he recommends. Our own rule is always to strive to apply the blades to the sides of the child's head, *whatever the position of the vertex may be;* for then we can make any amount of traction with a strong confidence that neither the mother nor child will suffer thereby. On the other hand, whenever the blades are applied obliquely to the child's head we make traction with many misgivings, and always expect to find the child more or less marked. In this connection may be mentioned the allusion of the author to the placenta-forceps "recommended by Dr. Bond, of *New York*" This carelessness may be excused in our British brethren, who waste no oil over Malté-Brun and the other Mystics.

In mento-sacral positions of the face the author recommends the application of the forceps. This advice has the sanction of high authority, but, to our thinking, it is extremely doubtful whether the handles can ever be pushed so far backwards as to permit the blades to be applied in the occipito-mental diameter, and their gripe would therefore be insecure and hazardous. Our own experience in such cases has not been large, but it has taught us to trust to the vectis as the safer and more efficient instrument.

In discussing the vectis, an instrument which justly claims far greater consideration from obstetricians than it usually obtains, just fourteen lines are required by the author himself. Four of them are consumed in describing its shape; five of them in *not* explaining whether its action is that of a lever or of a tractor, and the remainder in stating the cases in which "it *may* be used." He, however, adds a quotation of nearly one page from Dr. Coppée, which vaunts its efficiency in delivering the after-coming head. This may be the case when the life of the infant is not the first consideration, but not otherwise. From considerable experience in cases of retention of the head after the delivery of the body, we deem this section, as well as that one on breech presentation, to be faulty. Should the advice of Prof. Byford, or of his friend Dr. Coppée, be followed, too much precious time will be wasted in attempts at flexing the head, and at turning the chin into the hollow of the sacrum. Nor will it usually be necessary to introduce the fingers, and "plant them firmly on the cheek bones," for the greater friction of the occiput upon the pelvic walls will almost invariably bring about the requisite amount of flexion and rotation at the proper moment. In our opinion, after seizing the child by the neck and ankles, traction should be made first in the axis of the outlet, in order to depress the sacral side of the child's head, and cause it to be nipped by the promontory at the highest point possible. Then, without for one moment remitting this traction-force, its direction should be changed to that of the axis of the superior strait by firmly pushing the child's body backwards against the coccyx. This manœuvre extricates the head from

the brim not only by the leverage gained, but by causing its pubic side to describe a shorter arc around the promontory of the sacrum as a centre of motion.[1] Afterwards the traction-line must be accommodated to the curve of Carus.

Nor must the operator, in desperate cases, shrink from using very great force, a force indeed only just short of breaking the neck. It is emphatically "neck or nothing" in many of these cases of contracted brim; but fortunately it is astonishing what a strain the neck will bear without destroying life. "Traction of seven or eight pounds," observes the author, "is not considered very much; but I think very few of us would be willing to hold a child up by the neck after it was born." We, however, have repeatedly thrown all our weight possible upon the necks of children under such circumstances, assisted by external pressure upon the vault of the head, with the happiest results. In nine-tenths of breech cases it is the delay of the delivery and not the traction-force which kills the child. So important to the safety of the child is a rapid delivery, and so safely may very powerful traction be made, that, in pelves of ordinary size, we can conceive of breech cases, in which it would be justifiable to trust to Giffard's[2] and Frorieps'[3] plan of dragging the head through with the arms extended above it, rather than lose precious time in bringing them down.

We are pained to see, that the poor "student and busy practitioner" are expected to wait the canonical hour for something to turn up before proceeding to deliver a refractory placenta. For our part, we have hardly seen one delivered spontaneously for several years, unless it was pulled out by a short cord. In our opinion, so soon as the uterus is well contracted, the placenta should be taken away. For then, it will be found in one of four conditions, either (*a*) wholly in the vagina; (*b*) partly in the vagina and os uteri; (*c*) loose in the uterine cavity; or (*d*) adherent. Under the first three conditions it must act as a tampon, and should therefore be promptly taken away. If adherent, it may produce hour-glass contraction, therefore we take it away without delay. By the way, any one who has met with a case of incarcerated placenta through the use of ergot, will have good reason to doubt the statement made on page 399, that, "it does not cause contraction of the circular fibres of the cervix, but rather relaxes them."

This book is so full of evidences of carelessness—to use a very mild term—that we seem never to have done with them. Thus, on page 438, "Some authors speak of the hysterical form of puerperal convulsions, but I think the term highly objectionable in such a connection. When hysteria does occur in pregnancy or parturition, it is attended with all its characteristics, is of no more importance than at other times, and as a complication is simply embarrassing, *more dangerous*, and is amenable to the remedies that usually cure it." How can it be "*more dangerous*"—we ask—if it "is of no more importance than at other times," "simply embarrassing," and "amenable to remedies"? This is sheer contradiction. Again, at page 384, he illustrates the danger arising from detachment of the placenta by the history of a woman who bled to death in forty minutes from "the bursting of a varix." So far as the detachment of the placenta is here concerned, she might just as well have been gored to death by a mad bull. Further, on page 358 he inserts a wood-cut, to

[1] For a more extended account of this method, see Trans. Philadelphia Obstet. Soc. in American Journal of Obstetrics, Nov. 1870.

[2] Cases in Midwifery, Case LXVIII. [3] Journal Générale, vol. xxiii. p. 460.

show the method of version by external manipulation, in which "the fœtus" (instead of the figure) "is divested of the uterus and the abdominal walls, in order the better to exhibit the method of applying force." At page 102 he says, "the *hypostatic* region becomes slightly flattened or depressed" in the first three months of pregnancy. Where is that? Finally, pages 137 and 138 contain, a fine instance of that figure in rhetoric (onomatopœia) by which the words convey the sound of the thing signified. By making his patient and his mother-tongue alike the victims, he vividly represents the destruction of the former, by the destruction of the latter, in this wise.

"At other times, months, and even years after the cessation of the signs of fœtal life *elapses*, and only as a consequence of some accident giving origin to inflammation, the above series of circumstances are initiated . . . . Long continued inflammation, very frequently occurring in the attempt to eliminate the fœtus, with its exhausting consequences, undermines the vital forces or *lead* to some cachectic developments that *causes* the death of the patient."

We must not be understood to say that there are no good things in this volume, but they are few, and generally better—certainly more grammatically—told in other obstetrical works. His remarks upon the dressing of the cord and of the infant are excellent. We commend him especially for giving up the belly-band—a tradition of the lying-in chamber which has been handed down from barbarism, and which we hope to see banished from every monthly-nurse's brain and basket. It has, in our opinion, caused more pouting and sore navels, more umbilical and inguinal herniæ in children than all other causes combined. With an experience of over three hundred cases treated without belly-bands, we can confidently recommend the following plan: So soon as the child freely respires, the still-pulsating cord is to be grasped at its umbilical insertion by the thumb and forefinger of the left hand, and cut of the usual length. It is then to be "stripped" of its blood, and of as much as possible of the jelly of Wharton—any blisters of this gelatin being nicked with the scissors and emptied. During this process of "stripping," the compression near the umbilicus is to be occasionally relaxed, in order to allow that portion of the cord contained within the cavity of the abdomen to collapse and empty its contents. Whenever the bleeding ceases, or the cord becomes flaccid, it is to be tied in the usual manner, and then left to dangle about freely, without any kind of dressing whatever. Treated in this manner, it dries up without any bad smell, and drops off without leaving a sore stump. At page 67 the author has drawn a most charming distinction between the vital and the organic contractility of the womb. So true and yet so simple, that we regret its length will forbid a transcript of the entire passage; and we also regret that the rest of the book has not been written in the same unambitious style and with the same pains-taking care.

In conclusion, to the reader we say: a perusal of this work will make you prize all the more highly your Hodge or your Cazeaux. To the author we express our hopes that he will call in this edition; engage a good proof-reader, and in future adopt the maxim, "*contrahe vento nimium secundo turgida vela.*" Finally, if undue harshness has crept into this critical proof-reading—we can hardly call it a review—it has been called forth by a deeply-rooted loyalty towards our own, our native medical literature, and by a jealous fear of any slur from abroad that may be cast upon its fair fame. If this sensitiveness has grown morbid, we here make our humble apologies. W. G.

ANALYTICAL AND BIBLIOGRAPHICAL NOTICES.

ART. XX.—*Hand-book of Medical Microscopy.* By JOSEPH G. RICHARDSON, M. D., Microscopist to the Pennsylvania Hospital; Secretary of the Biological and Microscopical Section of the Academy of Natural Sciences, etc. etc. etc. 12mo. pp. 333. Philadelphia: J. B. Lippincott & Co., 1871.

IN this "hand-book" the author treats, first, of the microscope, its instruments, apparatus, and manipulations; and, secondly, of the employment of that instrument in pathological observations. He tells us in the preface that it was written to "fully elucidate all those minute but important details which, because they seem so simple to the skilful observer, have generally been passed over without particular explanation. Especial attention has also been paid to pointing out the mistakes into which an inexperienced microscopist is liable to fall from errors of observation or deduction." How successful Dr. Richardson has been in this task it becomes our duty to inquire.

We shall notice the optical parts of the volume first, and then pass on to a consideration of those chapters treating more especially of the use of the instrument in clinical medicine. The author explains, briefly, the optical principles of the simple microscope, which, we believe, is not now employed in pathology, and which we are informed has been superseded by the compound instrument; but we fail to find any description of the principles of the compound microscope, which *is* used in anatomical research.

Several forms of the compound instrument, however, are figured, and their mechanical arrangements described. The cheaper forms of microscope, which the author recommends for the beginner, are supplied with French objectives, and where these magnify from 250 to 600, and are non-adjusting for thickness of glass cover, our experience, and that of most others who have worked with such lenses, have taught us they are worthless for all accurate observation. Even the second quality lenses of our own makers are known by all judges to be much superior to these low-priced, commercial French objectives.

If Dr. Richardson has aimed at giving the beginner information regarding our own makers of good and moderately low-priced instruments, he has been, perhaps, unintentionally forgetful of some distinguished manufacturers in New York.

In noticing, on page 25, the optical construction of our best high-power objectives, our author falls into an error, from which even a moderate acquaintance with the subject would have saved him. He tells us such lenses are composed of "three pairs of lenses, each pair a doublet." Now, all authors who have written on this subject—and we mention only Carpenter and Hogg, because their works are in the hands of all—state that the objectives alluded to by our author "consist of as many as eight distinct lenses, the front and back being *triplet* combinations, with a doublet between." These authors also tell us that even the $\frac{1}{5}$, $\frac{1}{10}$, and $\frac{1}{4}$ inch objectives are made in the same way. Even the 1 inch and $\frac{2}{3}$ have a triplet combination in the posterior lens, while the anterior only is a doublet.

Again, the author informs us on page 29 that "most eye-pieces are composed of two achromatic lenses," in which statement he goes far beyond the knowledge of makers or authors who are most familiar with the subject. The Huyghenian eye-piece, made of two plano-convex lenses, neither achromatic, is the form in use in all our microscopes. We can explain an error like this only by the fact that very many persons work with instruments year after year

without thinking or caring how they are constructed; but such persons never become skilful in manipulation, nor qualified to instruct others in that which they do not understand themselves.

In writing of the binocular microscope—an instrument which American physicians and microscopists are singularly uninformed about—Dr. Richardson betrays the usual unacquaintance with its construction. He says the effect is obtained by an arrangement of "prisms," placed just above the objective. By reference to any recent work on the microscope, or inquiry of those using the binocular instrument, the author would have learned that Mr. Wenham employs only *one* prism for binocular effect, and his form is the only one in use by all English and a few American physicians.

In his explanation of the means employed to obtain polarized light, the author is equally unfortunate. He tells us, "two crystals of Iceland spar" are used in the microscope for this purpose. Doubtful, indeed, would be the results thus obtained. Since Erasmus Bartholin first discovered the phenomenon of double refraction in Iceland spar, we have no record in the books of that substance having been employed in its natural form of crystallization to obtain polarization in the microscope. But Mr. Nicol succeeded in cutting the natural prism into two equal portions, by a section carried through the "acute lateral angle, and nearly touching the obtuse solid angle." These sections are then highly polished, and cemented together with Canada balsam, and in its new arrangement the Nicol prism transmits "only *one* of the two rays, into which an ordinary beam of light is made to divaricate by passing through that substance." Mr. Nicol's process is published in the *Edinburgh Philosophical Journal*, vol. vi., p. 83. Dr. Richardson thinks, moreover, polarized light "possesses no great practical value." In geology alone, such men as Allport, Sorby, David Forbes, and others find polarized light the only means of surely detecting, in some doubtful cases, the presence of the igneous or crystalline rocks. The vegetable histologist also well knows he has no surer means of detecting starch granules; and the application of polarized light by such men as Professor Boeck of Christiania—who was the first to apply this means of investigation to animal tissues—and Brücke, promises an extension of our knowledge in regard to the obscure nature of the sarcous elements of muscular fibre. Polarized light, when skilfully employed, reveals to the ophthalmic surgeon earlier changes in the density of the crystalline lens in man and the lower animals, than can be detected by other means. And in the diagnosis of diabetic urine we have no surer test than polarized light.

Our author gives directions in Chapter II. for dry-mounting. "The object—an itch insect, for example—is simply to be deposited on an ordinary glass slide, in a suitable position, a thin glass cover laid gently upon it, and a piece of colored glazed paper, perforated by a circular opening, and coated on its under side with mucilage, pressed down over it," etc. We might be surprised that directions like these should still be found in the books, did we not know from ample experience in this department of microscopical work, that carelessness and the crudest results are the rule, rather than the exception. Are we never to make progress in the important attempt to properly prepare and preserve microscopical specimens in a condition fit for scientific study? We do not despair, even, when these old myths are found perpetuated in modern books. An acarus, or other delicate organization, so put up, would be crushed out of all shape; and it is common experience with all *who have worked* in this line to find that fungi soon invade and destroy the victims of such work; neither can they be best displayed, even temporarily, in that manner.

All through this portion of his book the author presupposes much knowledge on the part of the beginner, and, although the camera lucida and steel disk of Sœmmering are recommended for drawing, yet nowhere in the volume are those accessories described. We regret that the author has not treated the subject matter of the first two chapters of this hand-book more fully and lucidly, and must confess that they do not realize our expectations, especially as they are written by one who occupies the honourable position of "Secretary of the Biological and Microscopical Section of the Academy of Natural Sciences." Doubtless, microscopical science in our city will admit of improve-

ment; but we cannot think that this work affords a fair representation of its condition.

Dr. Richardson is manifestly not acquainted with the elementary optical principles of construction of either our best high power objectives, or of our common eye-pieces; and it is, therefore, necessary to accept with cautious allowance, or with positive distrust, what he may say about the superior quality of the optical arrangements employed by himself.

In selecting the salivary corpuscle as the most accessible and best object for the beginner to practise upon in his microscopical manipulation, we are happy to say that the author has taken a step in advance of those writers who direct that a fibre of linen or cotton cloth, or a crumb of bread, should be chosen for that purpose. These last-named articles have no direct relation to human histological observation, while salivary corpuscles and epithelial cells must be identified by the would-be pathologist, and the directions given by the author for their examination, are very full and clear, and must be of value to beginners.

Five chapters are devoted to the examination of the urine. The appearances which may be detected in that liquid by the unassisted eye, are made the basis of classification: as, for example, light, and flocculent deposits; other flocculent deposits; dense and bulky deposits; scanty and granular or crystalline deposits, and substances which float in or upon that liquid. But as these chapters are mainly commentaries on the works of Beale, Golding Bird, and Roberts, which have taken their place in medical literature, we shall allude to them in order to make only this remark. Our author directs on page 68, that the urine be allowed to stand, after collection, "twelve hours," before examination. In allowing twelve hours, Dr. Richardson differs widely from other authors. Drs. Roberts and Beale direct that the urine should be examined within a few hours after its secretion, because some specimens of that liquid "pass into decomposition within a very short time after they have escaped from the bladder." Blood cells change quickly in alkaline urine, and vibriones and bacteria sometimes swarm in such urine, in a short time after it has been secreted. "In those cases in which uric acid or octahedral crystals of oxalate of lime are present, the deposit increases in quantity after the urine has stood for some time. These salts are frequently not to be discovered in urine immediately after it is passed, but make their appearance after a few hours."

Even that amœboid motion, which seems to have been such a glorious revelation to our author when seen under his $\frac{1}{25}$, we are are told on page 85, may be observed in the white blood corpuscles in the urine of cystitis "if examined while fresh." Again, on page 134, Dr. Richardson admits "the clinical importance of a sediment of uric acid occurring in urine after twelve hours' standing, is very slight," and doubtless he is correct in this admission.

Our author devotes Chapter VIII. to the examination of saliva, mucus, pus, and milk; and the beginner finds himself, at once, plunged into the conflicting theories regarding the identity, or otherwise, of the salivary, mucous, pus, and white blood corpuscles. The author diluted a drop of his own blood on a slide under the microscope, with pure water; and he "found it quite possible to watch every step of the change in which, by *mere distension*, the white blood cell is converted into the salivary corpuscle, with its one, two, or three nuclei; its actively revolving molecules confined by a cell-wall of exceeding tenuity." Also, "when the liquor muci and liquor puris are similarly diluted, their corpuscles are also seen, for the most part, to be converted into salivary globules;" and the author "concludes, *therefore*, that most of the corpuscles of human pus are simply white blood-cells, which have wandered out through the vascular walls." Now, whether this idea of the identity of the Leucocytes of Robin shall be accepted or not, will depend upon the character of the evidence advanced in its favour; and while such doctrine is highly probable, we think the amount of proof in its favour is yet inadequate to justify its full acceptance. Moreover, the mere similarity *in appearance*, observed by our author, in these corpuscles, when under dilution by water, we deem entirely insufficient to warrant the conclusion that they are really alike in nature. The experienced embryologist well knows he cannot distinguish by mere microscopic inspection, the essential distinc-

tions between many cells which resemble each other in structure, but develop finally into organizations widely dissimilar. We are told, moreover, in *Stricker's Human and Comparative Histology*, vol. i. p. 13, that the "dancing movement ceases in the interior of the corpuscles of saliva, on the cautious addition of a solution of common salt, from one-half to one per cent. in strength, but this still permits the movement in fresh pus or lymph corpuscles to continue." We think the beginner would have found clearer directions for the identification of these important elements of pathological observations, had there been less theoretical doctrine mixed up with the admitted characters belonging to these corpuscles. And in writing hand-books for young students in the obscure departments of the medical art, it would be more desirable to present only the *established facts* of the science, than to become the prophet of doubtful things, at present only foreshadowed.

In treating of the examination of the blood, our author adopts the theory, older than the time of Schwann, that the red corpuscles are little bags filled with a coloured fluid; because this hypothesis explains better than any other the phenomena they exhibit.

The evidence brought forward in support of this view, we observe, is derived chiefly from observations on the dried blood of the Menobranchus; in which each dried corpuscle has its cell-wall apparently "propped out" by crystals formed within the cell. These intra-cellular crystals, Dr. Richardson figures in a very interesting paper on the subject, published in the *Transactions of the American Medical Association* for 1870. Now, while the appearances described by our author as being present in post-mortem blood corpuscles that have undergone coagulation with the fibrin in which they were enveloped on the glass slide, may have been apparent under the microscope; yet, it is by no means proved that those appearances are not susceptible of a very different interpretation.

The unsettled state of our knowledge respecting the red corpuscles of the blood is really remarkable, considering that physiologists have been examining into their structure ever since Swammerdam clearly detected their presence in the frog in 1658; and Leeuwenhoek in man, so far back as 1673, and it is more than probable they were known much earlier. The best observers of modern times, with rare exceptions, deny the existence of a cell-wall—that is, of a limiting membrane, structurally and chemically dissimilar from the other portions of the corpuscle. The German school of histologists teach the opposite doctrine; and Stricker and Alexander Rollett[1] do not assert the existence of a cell-wall. Robin, of France, and Dalton, and Flint, also deny its presence, though Draper writes of these corpuscles as "little elastic sacks." Dr. Beale is very clear in denying the existence of a cell-wall to the blood disks. We shall quote a paragraph from Dr. Beale, because he is quite familiar with the employment of high powers in this line of observation. "The red blood corpuscle of man and mammalia generally consists of a mass of soft viscid matter, perhaps of the consistence of treacle, composed of hæmatocrystallin. The outer part of this matter is of firmer consistence than the interior, especially in the older corpuscles. When the latter are placed in water, the more soluble matter is dissolved, leaving the harder external portion. By the action of many chemical agents, the outer part of the red blood corpuscle is condensed. These and other appearances have led observers generally to the conclusion that the red blood corpuscle was a cell containing fluid contents; and so firm has been the conviction that this is so, that the rupture of the 'cell,' and the escape of the contents, have been spoken of as if they had actually occurred, and had been seen. That the red blood corpuscle is not a cell, is proved conclusively by the following facts:—

"1st. A red blood corpuscle may be divided into many smaller portions, every one of which assumes the spherical or spheroidal form, and in many cases becomes stellate.

"2d. The mass of germinal matter, in the case of the nucleated blood cor-

[1] Human and Comparative Histology.

puscle of the frog, or a portion of it, may pass right through the red viscid material of which the outer part of the corpuscle is composed, without the rupture of any membrane, just as a solid particle might pass through treacle or molten pitch.

"3d. A red blood corpuscle from guinea pigs' blood assumes the crystalline form very readily, and without the addition of any reagent. The process may be watched under the microscope, and a single corpuscle seen to become a single crystal.

"4th. Several red blood corpuscles, under certain circumstances, run together, forming a soft homogeneous viscid mass; in which nothing like cell-walls, or the remains of such structures can be seen, and which undergoes crystallization in every part without exhibiting indications of cell-walls anywhere.

"5th. When water is added to blood corpuscles, they swell up just as a piece of jelly would swell up; but they do not burst, as is generally stated. No doubt soluble matters are dissolved out, but as the water evaporates, the corpuscles assume their previous form, although they appear paler than before. Although many appearances may be urged in favour of the existence of a cell-wall, the above facts, and those learned from studying the changes taking place during the development of the corpuscle, seem to be absolutely incompatible with such a view. Neither here nor elsewhere is the 'cell-wall' a necessary or essential portion of the elementary part. A thing may arise from a thing which existed before it; grow, live, perform its offices, and increase its kind without having a cell-wall at any period of its existence; and cell-walls may be easily made artificially, so that in spite of the importance hitherto attached to it, the 'cell-wall' is of no real significance."

That Dr. Richardson should have misinterpreted the appearances seen under his microscope is not so remarkable, since his Hand-book shows too plainly that he has not yet acquired a practical knowledge of his lenses, nor uncommon expertness in illumination or manipulation; and it must be regretted that the beginner should be misled by views based upon misinterpretations of appearances which are explainable in another manner, especially when taught by one who writes to "point out the mistakes into which an inexperienced microscopist is liable to fall from errors of observation or deduction."

In estimating the average number of white blood corpuscles in any given specimen of blood, our author truly says a "definite standard must be obtained," and he recommends to count the number to be found in five or ten "fields" of the microscope.

This is about as definite as it would be to attempt to measure the height of a man with a piece of tape. Persons really familiar with the microscope are aware that the size of the "field"—using the term in the sense intended by the author—depends upon the construction of the eye-piece, which our author seems not to understand, and the area of the field differs widely in instruments of different makers. The English and American microscopes have larger fields than those made in Europe, with few exceptions, and a specimen of blood, therefore, presenting only six white corpuscles when seen in an instrument having a small field, might show twice as many under a field with a wider area, and all comparative estimates, therefore, made by this plan, must be quite arbitrary, or entirely fallacious.

The remaining chapters of the book treating of sputum—vomited matters—discharges from mucous surfaces—parasites—the medico-legal examination of blood-stains, etc.—and morbid growths—may be consulted by the beginner with more advantage, because the author has combined the labours of others with his own experience, mainly free from theoretical views, and this is of great importance for those readers who are unpractised in difficult departments of microscopical research.

The superiority claimed by the author for his $\frac{1}{25}$ and $\frac{1}{16}$ we cannot think is made good in the book, for not one observation is there recorded but may be verified by lenses of longer focal length, and costing less money. The definition obtained by using lenses of such short focus, is "eminently delusive," and although they enable us to glimpse the dancing motion in a salivary corpuscle, yet if *observed accurately* each dancing granule is always surrounded with an

"unhappy luminosity" about its margin, which is unavoidable, because these lenses contain errors of spherical aberration, which forbid *perfect* definition. Histologists are resting satisfied with an *approximation* to distinctness of definition in their lenses, and this fact becomes apparent on the employment of deep eye-pieces, or of the achromatic amplifier; and it is improbable that there is one lens in America, at the present time, that will clearly define a Bacterium, though they may reveal lines on the diatoms more numerous than the eye can count. But a change is promised in this direction. Mr. Oscar Knipe, of this city, first proposed, in 1856, the use of negative achromatic lenses properly adjusted in the tube of the microscope, as a means of obtaining power with lenses of moderate focal length, and the plan has been in use until the present time, the obstacle to its extension being the imperfect correction of most objectives. By this means we are able to examine objects in shallow cells through moderately thick glass covers, and with a power and definition not to be obtained in any other known way. And since Dr. Royston-Piggott is calling the attention of opticians to these facts, in his various communications to the Royal Microscopical Society, and Mr. E. A. Dickerson, of New York, in a paper published in the October number of the *Science Gossip* for 1870, is also working in the same line, we feel hopeful that such an improvement will be made in lenses that any desirable power may be thus obtained, without the necessity of employing objectives of *extreme* shortness of focal length.

In his examination of sputum Dr. Richardson is the first to call attention to the square fracture exhibited by the elastic fibres of the expectorated pulmonary air-cells, and doubtless it is a diagnostic character of great value.

The volume is illustrated with thirty figures, ten of which are original; and has an index full enough for all purposes of reference and study.

We leave unnoticed a few literary errors, such as the grouping in one ponderous paragraph irrelevant matters, and—as we deem it—some defects in the arrangement of the subject. Indeed, extreme haste in preparation is manifested in its pages, and we therefore regard the book as one of those melancholy cases of premature delivery not altogether unknown to medical experience.

J. G. H.

ART. XXI.—*Microscopical Anatomy of the Human Liver.* By H. D. SCHMIDT, M.D., of New Orleans. 8vo. pp. 59.

THIS excellent paper on the minute anatomy of the liver, by Dr. H. D. Schmidt, formerly of this city, now of New Orleans, was originally published in the *Journal of Medicine* of the latter place, for October, 1869, and has now been issued as a separate publication. The author's researches are elaborate, and, evidently, have been conducted in a most careful and skilful manner, aided by mechanical appliances, evincing much knowledge and ingenuity on the part of the author. One of the most important results of the examination in the intimate structure of the liver is the discovery of two independent capillary nets in the lobules. One of these nets, the familiar vascular arrangement, which might be called for distinction the hæmal capillary net, as is well known, commences at the periphery of the lobules in the termination of the portal vein and hepatic artery, and ends in the central hepatic vein of the interior of the lobules. The other net, the biliary capillary net, consists of delicate tubes, intercalated with the hæmal capillary net, and ends in the interval of the lobules, in the commencement of the hepatic ducts. The hepatic cells occupy the meshes of the two intralobular nets, and exude their material as bile into the biliary capillary net. This view, we may say, confirms that of the most competent observers during the last decade. Better, we may say the researches of such observers, among whom may be mentioned Budge, Hyrtl, Frey, Chrzonszczewsky, Hering, etc., confirm the statement of Dr. Schmidt, for the writer attests to the discovery of the latter a dozen years ago, of the result above mentioned, and noticed in the number of this Journal for January, 1859.

Dr. Schmidt further announces as one of the important results of his investigations, that the lymphatics have their commencement in the biliary capillary nets. Though the drawings of the preparations appear to demonstrate this conclusion, the view appears to us so improbable that, before admitting it, we must wait for its confirmation by others.

As Dr. Schmidt has proved himself such an able and patient investigator, we hope he will continue his researches in the difficult field of microscopic anatomy. J. L.

ART. XXII.—*A Report on the Microscopic Objects found in Cholera Evacuations.* By TIMOTHY RICHARD LEWIS, M. B., Assistant Surgeon to Her Majesty's British Forces attached to the Sanitary Commission with the Government of India. Printed by order of the Government. 8vo. pp. 68. Plates XXIV. and three maps. Calcutta: 1870.

THIS brochure, which has been put forth under the patronage of the British Indian Government, gives another heavy blow to the asserted production of constitutional disease by fungi. So far as can be judged from internal evidence, its author is well fitted for his task, and, as an *attaché* of the Governmental Sanitary Commission, has had most ample opportunities of studying the disease, both as endemic in Calcutta and as epidemic in Bombay, Lucknow, etc.

After a concise statement of what has been found by various observers in cholera stools, Doctor Lewis first takes up the consideration of the cysts found by Budd, Brittan, Hallier, etc., and by them believed to be peculiar to the dejecta of the disease. He shows that there are not one but several distinct bodies; that some of them are the ova of acari, some the ova of intestinal worms, and some truly portions of cryptogamic plants. So far, however, from the latter (the cholera fungi of Budd) being peculiar they belong, as long since was shown by M. Busk, to a species of uredo, the bunt of wheat, and may be frequently found in coarse bread. Dr. Lewis, therefore, concludes 1, "that no cysts exist in choleraic discharges which are not found under other conditions," and 2, "that cysts or 'sporangia' of fungi are but very rarely found under any circumstances in alvine discharges."

The next subject which claims attention are the so-called "spores." These are shown to be divisible into four sets. 1. Globules, which are nothing but aggregations of fatty matter. 2. Blood cells, which have been effused, and are swollen by endosmose or otherwise altered. 3. Corpuscles, imbedded in a tenacious substance composing the well-known "flakes" of the rice-water stool. 4. Globular, inactive states of certain infusoria.

The only one of these about which there is any difficulty is the third class. These corpuscles are transparent, globular bodies, resembling white blood globules, and, under favourable circumstances, exhibiting movements "exactly similar to those seen in the blood lymph, chyle, and (so-called) mucous corpuscles." They are indistinguishable from the similar bodies found in the fluid of blisters, and "doubtless owe their origin to effused blood plasma, which assumes with great rapidity a low, ill-defined, and non-progressive organization."

With these corpuscles were certain globules, about which Dr. Lewis wishes to speak "with great caution." Frequently, he says, a globule has been observed for some time, and finally disposed of as being an oil one, when suddenly it is seen to protrude a portion of its substance; retract it, and while doing so another protrusion becomes visible at some other portion of the little mass, and then, perhaps, it will shift its position, exactly after the manner of an amœba. These are frequently hyaline in a fresh stool, but generally granular; no trace of a nucleus or contractile vacuole can be observed. Sometimes they are very numerous; but when there are other corpuscles in the field, which act in a somewhat similar manner, it is impossible to say to which class they belong, unless, indeed, they move across the field like an ordinary amœba, and

not merely content themselves with protruding portions of their substance into the surrounding fluid, as was stated the corpuscles last described did. Dr. Lewis thinks, though not confidently, that these bodies are the amœboid condition of a protozoon, and that they are the "swollen spores" of Hallier. In this connection Dr. Lewis expresses the opinion, based on many hundreds of examinations, that debris of columnar epithelium is very rarely found in cholera stools passed during life, although often abundant in the fluid found in the intestines after death.

A somewhat peculiar protozoon was frequently found, both in the motile and still state, in the dejecta of cholera patients, but was by no means always present, and was also observed in the watery passages produced by the action of a purgative upon a healthy man. Dr. Lewis, therefore, concludes that there are no spores or animalculæ at all peculiar to the stools of cholera.

It is well known that Hallier's theory of cholera differs from those preceding it, in asserting that the fungus—the essential cause of the disease—exists in the stools chiefly, if not entirely, in the form of exceedingly minute particles, which are without definite form or character, and yet are capable, under favourable circumstances, of developing the matured plants of a peculiar fungus, the most perfect type of which is parasitic on the rice plant; or, in other words, that these minute particles (micrococci) are the minute germs of such fungus.

The readers of this Journal can well estimate the value to be attached to Hallier's culture experiments from the results of Dr. Billings' repetition of some of them, as published here not long since (Oct. 1869, p. 520), and are therefore prepared to learn that Dr. Lewis, after carefully repeating the culture of the stools after the manner of the German *savant*, failed entirely to find any plant developed which may not be readily produced from other similar materials.

Further than this, Dr. Lewis has been fortunate enough to develop the exact fungal forms figured by Professor Hallier as peculiar to cholera, and that, too, not merely from the stools of the disease, but from those of a healthy individual, and therefore concludes "that no especial fungus has been developed in cholera stools, the fungus described by Hallier being certainly not confined to such stools."

It has been supposed by some that the stools of cholera patients contain a very large number of monads, bacteria, vibriones, and similar forms; but Dr. Lewis states: "Thus far I have not found this to be the case; indeed, the discharges of cholera patients, if examined immediately, do not contain such quantities of these minute bodies, especially if the stools have been voided in rapid succession, consequently have not been long detained in the intestinal canal. Neither have I been able, after repeated observations, to find that, during the decomposition of a cholera discharge, a greater number of the minute bodies associated with putrefaction were developed in it, than were developed under similar circumstances in ordinary alvine discharges; nor have I been able to find that any peculiar growth, animal or vegetable, will proceed from the one which does not proceed from the other." On this point, however, Dr. Lewis thinks his observations have been too few for him to speak decisively, and in his summing up says, "that no sufficient evidence exists for considering that vibriones, and such like organisms, prevail to a greater extent in the discharges from persons affected with cholera than in the discharges of other persons, diseased or healthy; but that the vibriones, bacteria, and monads (micrococcus) may not be *peculiar in their nature*, for these *do* vary, may not be the product of a peculiar combination of circumstances, and able to give origin to peculiar phenomena in a predisposed person, is 'not proven.'" To which the reply naturally subjects itself, neither is it proven that they have such origin and powers, and that the burden of proof certainly lies upon those who believe they have—not upon those who believe they have not.

In the latter part of the book Dr. Lewis, after stating the theories of Pettenkofer and Buchanan, in regard to the connection of cholera with the soil and water, gives some observations made by himself at various places in northwestern India on this point; but these are confessedly so imperfect that it is not necessary here to discuss them; and we close, hoping he will, as he hints, at some time in the near future, make an elaborate report upon the subject.

This volume is a most valuable contribution to our science, and in the highest degree creditable to its author, for the carefulness and industry he has displayed in his investigation and his great caution in drawing conclusions.

H. C. W., Jr.

Art. XXIII.—*Germ Theory of Disease.*

1. *Disease Germs; their Supposed Nature; an Original Investigation, with Critical Remarks.* By Lionel S. Beale, M.B., F.R.S., Fellow of Royal College of Physicians; Physician to King's College Hospital, etc. etc. 12mo. pp. 82. Philadelphia: Lindsay & Blakiston.
2. "*On the Real Nature of Disease Germs.*" By Lionel S. Beale, F.R.S., etc., in "The Monthly Microscopical Journal" for October, 1870.
3. "*Medical Aspects of the Germ Theory.*" Two Lectures by Dr. B. W. Richardson, *Med. Times and Gazette*, Oct. 29th and Nov. 5th, 1870.

The views advanced in the volume, the title of which is at the head of this notice, have been held by Dr. Beale for several years, while the facts upon which they are based have been referred to in various papers and lectures during the past seven years. Dr. Beale declares that although the facts are conclusive as regards the living state of the active matter of contagious disease, he is quite unable to subscribe to the arguments advanced in favour of the *Vegetable* Germ Theory of Disease.

The object of the present volume is to determine the nature of the supposed *germs* to which contagious maladies are now attributed.

In this book Dr. Beale first applies the term *Bioplasm* to what in his other publications he calls *germinal matter*, a substitution which has evidently much to recommend it. The mode of origin and growth of the Bioplasm, and that of the formed material resulting from it, are briefly described in the introduction. After this the subject of vegetable germs is taken up and illustrated, first by the *yeast fungus*, which is described as consisting, under a power of 1000 diams., of distinct *oval bodies*, with bud-like projections subsequently to be detached, *minute particles* (detached germs), and a fluid in which both are suspended. These *germs* and *germules* are composed of a central living *bioplasm*, and external *formed material* constituting a *cell wall*, but originating from the bioplasm only. Many of the germules are less than the $\frac{1}{100000}$ of an inch in diameter; but capable of growing like the parent cell, and giving origin to countless descendants. Most important in this connection, is the fact that these little particles are so protected by the surrounding formed matter from the influence of destructive agents, as to retain their vitality for a great length of time.

These spores are also so light as to be easily transported in the atmosphere, and thus constitute *germs in the air*. Certain of these are figured as magnified by the highest powers. Bodies 100 times heavier than these may be suspended in air. "Hence, it is not to be wondered at, that millions of these should exist almost everywhere; floating in the atmosphere, deposited upon everything, and ready to undergo development wherever the conditions may be favorable." (p. 21.) And though millions be destroyed, a single one may develop under suitable circumstances.

With regard to the *detection* of vegetable germs in the air, this was accomplished more than twenty years ago, and by different methods which are detailed.

Referring to Dr. Tyndall's experiment, in which he confirmed the existence of organic particles in the atmosphere, by proving it to be partially combustible, and also that the same truth may be shown by microscopic examination of the dust of the air, Dr. Beale says, no one would dare to make an assertion that the dust of the air *consists* entirely of disease germs, though remarks have been made in scientific journals which have undoubtedly led the public to believe that such an assertion is intended. Prof. Tyndall is also accused of having led people to suppose, "by his discoveries, surmises, assertions, and predictions published

in the '*Times*,' that dust consists of germs, and that air teems with animalcules and disease-producing particles, which is not really the case."

The very *minute vegetable organisms* which may be obtained from the air, or developed in animal and vegetable infusions, are described as being for the most part of an oval form, frequently exhibiting a constriction which corresponds to a point of division. There is, however, no possibility of identifying the different kinds of bioplasmic matter under the microscope. Animal or vegetable, they resemble each other, and they cannot be distinguished from particles of germinal matter derived from the living mucus or pus, or white blood corpuscles of man himself.

Dr. Beale denies heterogenesis—the doctrine that living things proceed from lifeless matter. The experiments of Dr. Child in 1864–65, are detailed. In the fluids, in the examination of which with high powers Dr. Beale assisted, *organisms were found* after the lapse of a variable period. He answers the argument derived from their presence, by the reply that, first, there is nothing to prevent us from coming to the conclusion supported by so many facts that are well known, that the living forms discovered sprang from the *living* matter which *resisted* the high temperature to which they were exposed.

Second, that it does not appear that *every part* of the apparatus of Dr. Child was subjected to the same high temperature. Moreover, we do not know everything concerning the influence of both high and low temperature upon the minute particles of bioplasm constituting the germs of the lowest forms of life. Some organisms live for a length of time imprisoned in solid ice, while there may be organisms which possess the remarkable property of resisting the destructive influence of a temperature of 300° or 350° F., that to which Dr. Charlton Bastian exposed his fluids. Indeed, he considers it more reasonable to suppose that the living forms discovered, arose from pre-existing germs, which obtained access in consequence of the arrangements being not quite perfect, than that they had been formed anew from lifeless matter.

As to the *manner in which vegetable germs might enter the body*, it is alleged that particles so minute could easily pass into the blood through the soft mucous covering of the mouth or stomach, entering these recesses with food and water. They would extend inwards, not as an amœba, but by growth and free multiplication in the substance of the epithelium, as the result of which they would get nearer and nearer the blood which they would finally attain. Here immersed, they would grow and multiply rapidly. Probably there is not a tissue in which these germs are not present, nor is the blood free from them. But as to their *relation to disease*, so long as the higher living matter lives and grows healthfully, the vegetable growths, though they may be, and are present, *they are passive and dormant;* but when changes occur, they become active and multiply. We must conclude, however, that their presence is due to alterations in the fluids *consequent* upon morbid changes; not that they are themselves the cause of disease. Indeed, "they cannot be regarded as the cause of the disease any more than the vultures which devour the carcass of a dead man can be looked upon as the cause of his death."

In the recent paper of Dr. Beale, "On the Real Nature of Disease Germs," after describing the structure, size, and form of disease germs as met with in the simple exudations, in the pus of purulent ophthalmia, gonorrhœal pus, vaccine lymph, variola of fever, syphilitic disease germs, disease germs in the secretions, and finally cancer germs, he concludes as follows:—

"These resemble one another in general appearance. Neither by its form, chemical composition, or other demonstrable properties, could the vaccine germ be distinguished from the smallpox germ, or the pus germ from either. All resemble the minute particles of bioplasm of the blood, from which they have probably been derived; but from which they differ so remarkably in *power*. Concerning the conditions under which these germs are produced, and of the manner in which the rapidly multiplying matter acquires its new and marvellous specific powers, we have much to learn, *but with vegetable organisms the germs have nothing to do. They have originated in man's organism.* Man himself has imposed the conditions favourable to their development. Man alone is responsible for their origin. Human intelligence, energy and self-sacrifice

may succeed in extirpating them, and may discover the means of preventing the origin of new forms not now in existence."

These are also nearly the views of Dr. B. W. Richardson, upon the same subject as expressed in two lectures "On the Medical Aspects of the Germ Theory," delivered before the Medical Society of London, and published in the *London Medical Times and Gazette* for Oct. 29th and Nov. 5th. 1870. They are, indeed, the only views in connection with the germ theory, which we consider at all justified by reliable observation and experiment. J. T.

ART. XXIV.—*The Pathology and Treatment of Venereal Diseases: including the Results of recent Investigations upon the Subject.* By FREEMAN J. BUMSTEAD, M.D., Professor of Venereal Diseases at the College of Physicians and Surgeons, New York, etc. etc. Third edition, revised and enlarged. With illustrations. 8vo. pp. 704. Philadelphia: Henry C. Lea, 1870.

PROF. BUMSTEAD'S work is so well known to the profession both abroad and at home, and has so long borne the deserved reputation of being the best treatise on venereal diseases extant in the English language, that the reviewer's task upon the appearance of a third edition might well be limited to a mere announcement of the publication of the volume, no words of commendation being required to secure for it the favourable reception which it is sure to meet with. The whole work, the author tells us in his preface, has undergone a thorough revision, and many additions have been made, the chief of which refer to (1) the treatment of stricture, due attention being given to the operations of rupture and of internal urethrotomy, (2) the subject of visceral syphilis, and (3) that of the syphilitic affections of the eyes, the chapter on the latter subject having been revised by Dr. E. G. Loring, of the Manhattan Eye and Ear Infirmary. A careful inspection of Dr. Bumstead's new edition has satisfied us that the promises of the preface are amply fulfilled in the body of the work, and that the high reputation acquired by previous issues[1] will be worthily maintained by that now offered to the public.

In reading Dr. Bumstead's pages a few points have seemed to us to call for further comment, some of theoretical, some of practical, and some of historical interest; but, in calling attention to them, we beg it to be understood that our criticism is offered in no invidious spirit, and with a full sense of the importance and great value to be attached to any opinion or assertion of the distinguished author. In the first place, we cannot but regret that Dr. Bumstead gives even the very qualified support which he now does to Clerc's view that the chancroid is a derivative of the chancre: this is a question which is to be considered not merely in the light of a few observations made in the practice of syphilization, but of all that is known both of the *natural history* and of the *history* of the two diseases. Indeed we are surprised that Dr. Bumstead does not himself see the inconsistency of devoting, as he does, a number of pages to proving that the chancroid was and the chancre was not known to the ancients, and then admitting even the possibility that the former may be a derivative of the latter: surely the derivative cannot have preceded in time that from which it was derived.

On page 52 we find it asserted that the chancroid is never met with upon the

[1] We trust it may not be considered unbecoming to refer in this place to the very strong resemblance borne, in some parts, to the early editions of Dr. Bumstead's treatise, by a work on Syphilis, Chancroid, etc. recently published by an Irish surgeon, Dr. J. K. Barton, of Dublin, and which has been very favourably reviewed by numerous critics. This resemblance, which we do not remember to have seen pointed out hitherto, is sufficiently evident on a careful comparison of the two works, and, while we can scarcely believe it to be the result of an accidental coincidence, is, we think, closer than is justified by the not too respectful reference to Dr. Bumstead's labours found in the author's preface.

head or face, although it may be implanted there by artificial inoculation; a statement which is scarcely reconcilable with the facts of Puche's and Rofeta's cases quoted on page 328.

In speaking of Vidal's plan for the treatment of gonorrhœal epididymitis (page 144), Dr. Bumstead refers to Demarquay's unfortunate experience, but does not allude to similar cases recently reported by Salleron, while the *favourable* results recorded by other modern writers are equally ignored; the criticism upon Mr. Henry Smith, of King's College Hospital, is, we think, unnecessary, since that gentleman has, if we mistake not, honestly acknowledged the priority of the French surgeon in introducing the mode of treatment in question, in a more recent paper than his original communication in the *Lancet* for 1864. Mr. Rouse's late proposal to cure swelled testicle by the administration of opium pills is not mentioned—perhaps is not deserving of mention—but we should like to have the opinion of such an authority as Dr. Bumstead upon the subject.

The chapter on gonorrheal rheumatism does not strike us as being quite up to date; we miss any reference to the discussions on this subject at the Société Médicale des Hôpitaux de Paris, and elsewhere, and should like to see some slight allusion, even in the way of contradiction, to the pathological theories based upon the undoubted analogies between this affection and pyæmia.

The question whether flexible or metallic catheters are to be considered preferable, is briefly dismissed with the sentence "Gum-elastic catheters, stiffened by a stylet, are sometimes employed, but are far inferior to those of silver." This question, in the abstract, is of course one of judgment and experience, and one upon which Dr. Bumstead's opinion is at least as valuable as that of any one else; but we must protest against the implied assertion that those who prefer flexible instruments use them with the stylet. For ourselves, we think with Sir H. Thompson[1] that "the gum-elastic or English flexible instrument is very valuable on account of one quality which it possesses, . . . *i.e.*, it will preserve any curve when cooled which" may be applied to it "under the influence of heat." It is perhaps needless to add that we use the English catheter without the stylet.

Dr. Bumstead's views with regard to the use of mercury in primary syphilis have undergone considerable modification since the publication of his first edition, and he now only employs this drug in exceptional instances, believing "that those cases ultimately do best in which specific treatment is deferred until the secondary stage." We note this change of opinion with a good deal of interest, having been ourselves long somewhat skeptical as to the advantages to be derived from mercurialization in the treatment of the primary sore.

The chapters on visceral syphilis are well worth the attention of the student; we cannot, however, but regret that no reference is made to the early labours of Dr. Reade, of Belfast, in the remarks upon syphilitic affections of the nervous system.

We should like very much to have chapter and verse for Dr. Bumstead's quotation from Aristotle on page 341. This aphorism, of which, by the way, Dr. Bumstead gives only a portion,[2] and that by no means the most curious, is one with which we have long been familiar as a *facetia*, the first part of which is humorously attributed to Aristotle by Captain Grose in his *Classical Dictionary of the Vulgar Tongue*, but which we certainly never expected to see, in a work of the gravity and dignity of Prof. Bumstead's, given as a veritable quotation from the Stagirite. It occurs also in that remarkable production of the celebrated John Wilkes, the "Description of Medmenham Abbey,"[3] and, if we mistake not, in the legend of one of Hogarth's plates, but further than this we have never been able to trace it. Some years since, when we had more leisure for classical pursuits than at present, we made, in conjunction with that well-known scholar and antiquarian, the late Mr. John Penington, a careful and persistent though fruitless search for any part of the sentiment in question,

[1] Clinical Lectures on Diseases of the Urinary Organs, Phila. 1869, p. 50.

[2] The whole is: "Omne animal post coitum triste est, præter gallum gallinaceum, et sacerdotem gratis fornicantem."

[3] The New Foundling Hospital for Wit, edit. 1786, vol. iii. p. 107, note.

through Aristotle, or rather through the Latin version of his works. for Aristotle of course wrote in Greek; and we may add that though the peculiarities of the "gallus gallinaceus" are freely discussed by Ælian, Pliny, and Columella, none of these writers make any allusion to its special cheerfulness under the circumstances in question.

Returning from this digression, we will terminate our brief and imperfect notice of Dr. Bumstead's volume, by saying that not only is it the best work on venereal diseases, in our language, with which we are acquainted, but one of the pleasantest to read. The author's style, always clear and forcible, is occasionally lit up by a picturesque or slightly humorous turn of thought or expression, which agreeably serves to prevent that sense of monotony which is so apt to attend the perusal of a scientific work, however excellent its doctrines may be.

The book is handsomely printed, and contains very few typographical errors. We do, indeed, find on one page Halli Abbas, Rhazis, and Albucasa, instead of Haly Abbas, Rhazes, and Albucasis, but for these mistakes the printer is perhaps not entirely responsible. J. A., Jr.

Art. XXV.—*St. Andrews Medical Graduates' Association Transactions*, 1869. Edited by Leonard W. Sedgwick, M. D., Honorary Secretary. 8vo. pp. 306. London: John Churchill & Sons, 1870.

The present volume is scarcely equal in the interest and value of its contents to its two predecessors, and we confess that our impression after reading it was that many of the papers in it had been written under pressure and apparently to insure its appearance. Even some of the best of the papers partake rather of the nature of reviews than of original essays on the subjects of which they treat. Dr. Richardson is still the president of the Association, and chose as the subject for his anniversary address "*The Science of Cure.*" Like his other addresses, this one is an admirable and well-written paper, clearly indicating many of the obstacles which have hitherto prevented medicine from becoming an exact science, and also the true methods of advancement. We shall, however, only quote from it the following remarks which he makes in regard to the existence of the *vis medicatrix naturæ:* "In my way of thinking the belief has, in the abstract, no basis whatever; by which I mean that nature goes her own way without putting out any hand to us for our special and particular aid. If nature were a curer, as is supposed, then all diseases were spontaneously curable, and all other curers than herself were impostors. But nature pursues her way with men as regardless of their infirmities as of their powers, her general course being towards some grand and unknown end in which the individual sinks into his true insignificance by the side of the vastness of her structures, work, design. * * * The term *vis medicatrix naturæ* is, in fine, an entire misnomer, except it be limited in application to the simple capacity possessed by the organism at given ages or periods of life to resist gravitation."

The Clinical Examination of the Urine in Relation to Disease. By C. Black, M. D., M. R. C. P., etc. etc.—The object of this paper is to show that important deductions are to be drawn from a daily examination of the urine in disease in reference to its quantity, density, colour, and to the nature and constitution of the deposits which occur in it, as well as of the adventitious products which are held in solution or suspension by it in some or all of the phases of disease. The paper, although well written, is unnecessarily long; all that is really important might have been said in half the number of pages.

The quantity of urine is found to be increased in hysteria, in neuralgia, in the beginning of fevers, in diabetes, in the beginning of cirrhosis of the liver, and of hypertrophy of the heart, and diminished in the hot stage of fevers, in the

more advanced stages of cirrhosis of the liver, and of hypertrophy of the heart, and it is also here said in all diseases of the kidney of an exudative character. An increase in quantity after a diminution is favourable, as it shows that the disease has reached its acme; on the other hand, if this increase does not take place, an unfavourable prognosis may be made.

The density of the urine is reduced in the different nervous affections, in granular degeneration of the kidneys, in the phosphatic diathesis, in the cold stage of fevers, and in many instances in that period of collapse which ushers in local inflammation. The tendency in all diseases—and this is especially true of inflammations—as they approach the termination, is to give rise to an increase of the specific gravity of the urine, except in granular degeneration of the kidney, the influence of which is, from first to last, to lower the specific gravity of that secretion. If the disease has no disposition to recovery, the increased density will be maintained. In phthisis pulmonalis fluctuations in the density are observed, and we are frequently able to draw important inferences from them; thus, a high specific gravity, with only slight diurnal fluctuations, indicates an acute form of the disease; a lower specific gravity, with a greater daily range, a chronic form, while a density nearly normal occurs in those cases in which the disease is stationary. In diseases of the digestive organs, as well as in diseases which are accompanied by disturbance of these organs, the specific gravity of the urine will be found to present variations; in most cases it will be increased, but if the disease be attended with much pain, it will be diminished. In cancer of the stomach, in cancer of the liver, and in hypertrophy, cirrhosis and abscess of the latter organ, it is increased; and this is also the case in any disease involving a mechanical obstruction to the flow of bile. When heart disease is not producing any secondary results the density of the urine may remain natural for years, but whenever it is attended by marked congestion then the density will be increased. Abdominal tumours, if benignant, do not occasion any marked alteration in the density of the urine, but malignant growths, probably from their more rapid growth, are attended by an increase. The colour of the urine is rendered pale by nervous diseases, by diabetes, by granular degeneration of the kidney, and by the phosphatic diathesis. It is milky whenever it contains pus or chyle. On the other hand, in oxaluria, in active inflammation, in fevers, the urine is of a yellowish or brownish-red colour. In acute rheumatism the urine has a peculiar lake colour due to phosphate of ammonia; and this peculiar colour of the secretion may sometimes enable us to recognize the rheumatic origin of pain. A tendency of the urine to become lighter in colour in the course of acute disease is to be regarded as favourable.

Dr. Black also attaches importance to what he calls the aërability of the urine, or in other words its capability, "on being shaken in a bottle, to maintain a frothy head, caused by air imprisoned in the form of bubbles on its surface." Urine holding in solution sugar, albumen, or bile possesses this property in a marked degree, which is attributed to the cohesion which these substances impart to the film which incloses each air bubble, and which enables it to resist the expansive power of the air within it, and when the fluid is exposed to the atmosphere the pressure of the latter upon its surface.

The occurrence of the urates as a deposit in the urine indicates either an excess of food or of disintegration of the tissues, and it is of course important that this distinction should always be made. In diseases of the organs of digestion, and in such as produce rapid disintegration of the tissues, there will consequently be found deposits of these salts. In an acute disease a deposit of urates is generally indicative of a tendency to recovery, but in wasting diseases, as phthisis, their amount is simply a measure of disintegration. They occur during the convalescence of inflammation, and depend probably upon the absorption of the exudation, and hence will be deposited for a longer time after inflammation of a serous than of a mucous membrane, because in the former case there is no direct outlet for the exudation as in the latter. If after the eruptive fevers have attained their height no urates are observed in the urine, complications or sequelæ are to be feared; or if in acute gout or rheumatism the improvement of a joint is suddenly followed by the disappearance of the sediment from the urine, another joint will soon be involved. If the urates

appear in the urine during the course of albuminuria or of diabetes mellitus, it is an indication of improvement in the condition of the kidneys. The urates will be reddish in colour in inflammatory diseases and in functional disturbance of the digestive organs; pinkish in acute articular rheumatism, but whitish whenever there exists nervous irritation rather than inflammation. Uric acid is not frequently found in the urine in inflammatory disease, but is deposited in muscular rheumatism, in diseases of the heart, and in diseases of the stomach accompanied by an excess of acid.

On Haschish. After Professor Polli, of Milan. By B. W. RICHARDSON, M.D., etc. etc.—Dr. R. details some experiments made by Prof. P. upon himself and two friends with this article. About 1½ gramme was taken by the mouth in the course of a little more than a half hour, and at the same time ½ gramme mixed with tobacco was smoked. It is mentioned that the temperaments, modes of life, and occupations of the three experimentalists were different. The following is the description given of the effects produced: "The symptoms of inebriation from the haschish once pronounced, developed themselves with great rapidity in them all. Their first sensations were of intense astonishment at the circumstance that they found themselves no longer masters of their own acts, while they still remained lucid witnesses of all acts, however foolish. Here the difference between alcoholic inebriation and inebriation from haschish is strongly marked. They saw themselves committing absurdities of the most grotesque kind; leaping, beating time to nothing, moving their arms as if receiving electrical shocks, writing ridiculous words, and so forth, without any power on their part to prevent such exhibitions, but yet standing, as it were, independent of them, as though they were merely subjects of observation exhibited for other persons than themselves. At first they had the sensation and appearance of feigning a state of exaltation which they did not feel, and which was even feigned with so much uncertainty and awkwardness that any one who would not be aiding in it would for a long time believe in its unreality. It is, nevertheless, an irresistible propensity. * * * At one moment the intellect is obscure and loses itself in forgetfulness of the past; and then it returns clear, and is able to form a judgment for a moment, and to disapprove of any acts it may have before sanctioned, but only to be again involved in that state of automatic folly which is so peculiar a phenomenon during haschish intoxication. During the intervals of confusion or darkness, the lucid moments possess a power and comprehension truly marvellous: so that in a few seconds the most distinct and accurate picture of a range of life including as much as forty years may be recast and surveyed. * * * That seeming extraordinary slowness of time, which struck the observers in so singular a manner, and made them so impatient of delay that they were continually recurring to their watches, and observing with a kind of awe, how minutes were transformed into great epochs, is explained by Dr. Polli as due to the rapid and varied succession of the numerous ideas which passed through the mind. * * * There was another singular condition excited by the narcotic in our experimentalists—a moral phenomenon: the influence of the drug showed itself in exciting in the observers, so different themselves ordinarily in general character and temperament, a common docility and absence of susceptibility which was most remarkable. Thus one of them gave to another, with whom he was but slightly acquainted, a series of hard blows on the back, saying that he himself felt nothing of the haschish, and asking whether the blows inflicted were felt. On his part, he who received the blows took them all in good part, uttering no complaint, and seeming, indeed, unsusceptible of complaint. Again, one of them who sat writing submitted to receive the infliction of two sharp blows, boxes on the ears, and to have his pen snatched out of his hand, without any expression of pain or even annoyance."

Dr. Polli reports that symptoms resembling those of hydrophobia were induced in his own case; thus he was seized with fear at the sight of bright objects, or at the sensation of every sharp little breath of air, or at the approach of any one, and was unable even with the greatest effort to swallow a single draught of water. He also felt an unpleasant sensation at the back of the

head, before the occurrence of convulsive movements, and one of cramps in his legs.

Dr. Polli has, since these experiments, administered this drug to a lady affected with melancholia with a tendency to suicide. In ten days the patient had taken fifty grammes, and it is said with a steady and progressive amelioration of all the phenomena. Her nights became tranquil, her intelligence just, her affections natural. The cure was permanent. Haschish was also given in a case of hydrophobia with the effect of mitigating the patient's suffering, or to quote the words of the writer, "it changes a raving, unmanageable, suspicious, aggressive maniac, who bites and who curses you, into a poor invalid, content and tranquil, who blesses you." The drug is said to act most quickly and powerfully when administered in infusions of tea, coffee, or cocoa, but its effects are very much diminished if it is given in combination with a vegetable acid.

Notes on the Therapeutic Value of the Chloride of Ammonium. By WILLIAM CHOLMELEY, M. D.—Dr. C. appears to be impressed with the value of chloride of ammonium in doses gr. xv to gr. xxv as a remedy, chiefly in various forms of neuralgia, in tic douloureux, hemicrania, nervous headache, myalgia, lumbago, and in amenorrhœa and dysmenorrhœa. In the two last diseases its activity is increased by adding iron to it. In regard to its mode of action he says, "I believe that it acts primarily on the sympathetic nervous system, on the ganglia, and on the vaso-motor nerves, and that through them it affects the sensitive nerves, the spinal cord, and the brain, as well as the other structures and organs of the body.

Remarks on Therapeutics. By WILLIAM PROCTER, M. D., F. C. S.—Dr. P. holds with Niemeyer, that in the existing state of knowledge the true test of the value of therapeutical remedies must be clinical experience. Without denying to physiological experiments their value and importance mainly from being suggestive, it is evident that the final decision will depend upon the result when submitted to the clinical test, which must be carefully applied with due regard to all concomitant circumstances.

Aphasia and its Seat. By S. LAWRENCE, M. D.—After a few introductory remarks on this subject, which, however, do not possess any novelty, Dr. L. relates the history of a case of abscess of the brain, involving the left frontal lobe, and consequently Broca's convolution, in which aphasia was nevertheless not a marked symptom. A boy, aged 12 years, was injured in the head by a fall, in May, 1863, and was, in consequence, several weeks under treatment. There was at this time neither paralysis nor unconsciousness, and his speech was only partially affected, for he was able to answer questions correctly, although he did so slowly and in monosyllables. After his discharge from the hospital his memory and power of attention were found to be impaired, and on any slight attack of illness supervening, more or less temporary unconsciousness occurred. All defect of speech, however, had disappeared. In the middle of March, 1868, grave cerebral symptoms set in, including insensibility, and speechlessness to a considerable extent.

Dr. Lawrence believes that the parts of the brain involved in the abscess were in a diseased condition from the time of the reception of the injury to the date of his death, during the greater part of which period there was no defect of speech.

On Gastric Neuralgia. By W. H. DAY, M. D., M. R. C. P.—This paper does not contain much that the members of our profession in this country are not thoroughly familiar with. Dr. D. deprecates trusting alone to drugs in the treatment of gastric neuralgia. In regard to the use of bismuth he says: "When the tongue is reddish at the tip, furred in the centre, and the papillæ prominent, particularly at the base, where they are often seen flabby and large, denoting irritation and debility of the gastric mucous membrane, bismuth is a remedy of undoubted value." So also when there is an excess of secretion on the part of the stomach. On the other hand, when the tongue is clean or pale, or large, or flabby, or has a glazed or membranous appearance, it does positive harm. The citrate of iron and strychnia is an admirable remedy in the treatment of this disease, but care must be taken that it does not produce congestion of the liver and stomach. The mineral acids frequently will act like a

charm in cases in which there is congestion of the liver attended with pyrosis, epigastric pain, or weight and incessant sickness. The bitter tonics may be given if the stomach is not irritable. The black oxide of manganese seems to be possessed of similar properties to bismuth, and is indicated in the same conditions, but unfortunately, like bismuth, it has no power to prevent a return of the complaint.

Cases of Melanosis. By W. NORRIS, M. D.—Dr. N. reports seven cases of melanosis, one of these, from the number of organs affected, is of unusual interest. He is inclined to think that the disease has increased in frequency lately, and has, in some measure, kept pace with the fashion of smoking, and with the wonderful increase of ironworks and manufactories. He believes, moreover, that it is closely allied to cancer, and that frequently, in a family of a patient with melanosis, it will be found that one or more individuals have suffered with cancer. He reports that the disease sometimes takes its origin from moles, and in this connection says: "There are several species of moles, and I think they will excite more attention than they have done. Some are small, of a pale-brown colour; others are larger, and very black, and these are the most likely to run into obstinate disease if irritated. When disease begins in or near a mole, I think it is almost sure to be malignant." In the case already alluded to the disease originated in a mole, and gave rise to a tumour, which was removed. The heart and lungs, the dura mater, and all the organs of the abdominal cavity showed the presence of the deposit. In another case sudden death took place in the midst of apparent health, and was explained by the discovery of the existence of melanotic disease of the lungs. In a third case a melanotic tumour was developed in a mole which had been irritated. There was no return of the disease in this case after the removal of the tumour.

A Study of Convulsions. By J. HUGHLINGS JACKSON, M. D., F R. C. P.—Dr. Jackson begins this paper, which is an elaborate one, by saying that "a convulsion is but a symptom, and implies only that there is an occasional, an excessive, and a disorderly discharge of nerve tissue on muscles." Convulsions, he thinks, may be arranged in two classes.

1. "Those in which the spasm affects both sides of the body almost contemporaneously. In these cases there is either no warning or a very general one, such as a sensation at or about the epigastrium, or an indescribable feeling in the head. These cases are usually called epileptic, and sometimes cases of genuine or idiopathic epilepsy."

2. "Those in which the fit begins by deliberate spasm on one side of the body, and in which parts of the body are affected one after another."

The cases which form the second class are those which are specially considered in this paper, the object of which is to show that the most common variety of hemispasm is a symptom of disease of the same region of the brain as is the symptom hemiplegia; viz., the region of the corpus striatum. Palsy depends on destruction of fibres, and convulsions on instability of gray matter. It is not, of course, intended to assert that the convolutions are never to blame in severe convulsions; for, being rich in gray matter, it is highly probable that the discharge does in many instances begin in the convolutions; but the gray matter of the lower motor centres, even when these are healthy, will be discharged secondarily by the violent impulse received from the primary discharge. Much importance is attached to the fact that the corpus striatum and many of the convolutions are supplied by one artery—the middle cerebral. Fits beginning unilaterally may start by movement in any part of the region which is ordinarily paralyzed in hemiplegia, *i. e.*, in the face, in the arm, or in the leg; but especially in the side of the face, in the head, or in the foot, or, in other words, in parts which have the most varied uses. This is, perhaps, due to the fact that these are the parts which are represented in the central nervous system by most ganglion cells.

Convulsions of course occur in different degrees.

1st. "The spasm attacks only the unilateral muscles of the side in which it begins, say the right."

2d. "It passes on to the bilateral muscles of both sides."

3d. "It goes still further, and attacks the unilateral muscles of the other side—the left—and probably the bilateral muscles of both sides a second time."

It has been observed generally that when the spasm begins in the hand, that it passes up the arm, then attacks the face, and lastly goes down the leg —"out at the toes," as one of Dr. Jackson's patient's expresses it. If the foot be the part first attacked, the spasm passes up the leg and down the arm.

Dr. Addison believed that the absence of insensibility in convulsions was some evidence that the internal lesion was organic, as, for instance, a tumour; but although organic disease has generally been found at the autopsy of such cases the statement is too general, and Dr. Jackson holds that "such convulsions point only to minute changes involving instability in the opposite hemisphere." They of course tell us nothing of the pathological processes by which that local instability results—or, in narrrower terms, they give no information as to whether the changes are primarily minute or secondarily minute, that is, changes due to the irritation of a foreign body.

Dr. Jackson has observed in cases of convulsions that when spasms begin on the right side there is defect of speech more marked than when it begins on the left, and the defect of speech is attributed by some to some temporary change in some part of what he calls the Sylvian region, *i. e.*, the portion of the brain nourished by the middle cerebral artery, and this of course includes Broca's convolution.

In regard to the cause of convulsions beginning unilaterally, Dr. Jackson says that the word "cause" is used in various senses in medical language, and he therefore discusses the following points:—

1st. The seat of the internal lesion.

2d. The nature of the changes in nerve tissue on which the spasm directly depends.

3d. The pathological process from which these local changes result.

4th. The circumstances which may determine a paroxysm.

1. He holds that the fact that the symptoms are local, implies, that there is of necessity a local lesion, as surely as does one-sided palsy, and the fact that the patient is apparently quite well between the paroxysm does not negative this view in the least, for it is well known that a large part of one of the cerebral hemispheres may be destroyed, or extensive disease exist in it, without giving rise to obvious symptoms of any kind. But while there need be no symptoms if a portion of the convolution is destroyed, mere instability of this part will give rise to convulsions, for it will discharge on muscles when its tension reaches to unstable equilibrium. If the discharge be excessive, the nerve fibres will require some time to recover from this "overwork;" and in the meanwhile the muscles which have been convulsed are paralyzed. Even in those cases in which no lesion can be discovered on the post mortem, Dr. Jackson would still believe in its existence, for it should be recollected that even in those cases where a "*lump*" is found in the brain, that the convulsions have not been so much due to it as to the minute changes it has induced secondarily in the surrounding tissue.

2. Dr. Jackson believes that the nature of changes in nerve tissue, on which the spasm directly depends, is that of instability. And

3. That there are but two patholgical processes which lead to the local instability above mentioned, of which he can speak with any confidence, viz., (*a*) changes produced by embolism, and (*b*) changes diffused from coarse disease. As to the nature of the coarse disease the symptoms tell us very little, and the diagnosis must frequently be made from the history or from the symptoms of disease in other parts.

4. In reference to the circumstances which determine a paroxysm Dr. Jackson says there are two factors. 1st. Permanent local instability. 2d. Something which determines the discharge of the part unstable, and the exciting causes are various, for there are many things which will upset the equilibrium of highly unstable nerve tissue, as for example, excitement, flatulence, dyspepsia, fright, etc., all of which act by altering the circulation in the brain; but the equilibrium of the side diseased only will be affected because on that side only is

there nerve tissue which is in a condition to explode on slight and general provocation.

Points to be observed in Ovariotomy. By D. Lloyd Roberts, M. D., M. R. C. P.—This paper contains nothing which will be new to the surgeons of this country at least.

Prurigo and Pediculosis or Phtheiriasis, and their Connection with Pediculi. By Tilbury Fox, M. D.—Dr. F. is anxious to demonstrate in this paper that the lesions generally termed pruriginous may exist independently of the irritation caused by lice. The true papulæ of prurigo are formed by deposits of lymph in the skin and are new solid formations, and the black apex (hemorrhagic) which is seen does not result from scratching. On the other hand, the papules which are peculiar to phtheiriasis, are caused by the pediculus introducing his haustellum into the skin for the purpose of drawing nourishment from it, and if these be carefully examined, a minute puncture will be discerned. In some cases these papulæ will be surrounded by a zone of congestion. When the skin is irritable, or where pruritus exists, scratching will give rise to appearances very closely resembling those of true prurigo. A great deal of the confusion on this point has arisen from a misconception of the nature of the injury inflicted by the louse, which does not bite as is generally supposed, being provided with a sucking apparatus and not with mandibles.

On Intermittent Pulse and Palpitation. By Benjamin W. Richardson, M. D., F. R. S.—This is one of the best papers in the volume, and we shall make a full abstract of it. In speaking of intermittence the author says the gap is due to a break or holding back of the ventricular systole, and instead of the normal rhythm of sounds as represented by lūb dŭp O, we have O dŭp O succeeded by lūb dŭp dŭp, and then a return of the normal succession of sounds. It is clear Dr. Richardson thinks that the failure of the first sound is due to the absence of the ventricular systole, and not to absence of blood in the ventricles, as there are both positive and negative proofs that there is no such absence. The occurrence of the second sound during the intermittence is due to the closure of the sigmoid valves of the pulmonary artery. The prolonged, dull, first sound which follows the intermittence is due to the vigorous effort which the ventricle makes to contract upon its blood. The reduplication of the second sound arises either from a separate closure of the pulmonary and aortic set of valves, or to a simultaneous double but feeble closure of both.

In regard to the organic cause of intermittence of the pulse, the author is decidedly of the opinion that it must be sought elsewhere than in the heart. All the evidence goes, he says, to prove that in every case of true intermittence one particular point or centre of the nervous system is the primary seat of the derangement, and this derangement might be in the ganglionic centres of the heart itself, or be due to irritation, in the periphery or in the branches of the pneumogastric, or in some mental centre of the nervous system. It cannot be, he thinks, in the first, for were it, the nutrition of the heart would surely be more decidedly influenced, and the cardiac symptoms would be not intermittent, but persistent, nor can it arise from the second, for it could hardly be continuous for years, with no other sign of muscular disturbance. The clinical history always points to the third as the true cause. Dr. Richardson has never met with a case, in the prime of life, which has not been traceable to some form of mental excitement with succeeding depression. In two cases intermittency of the pulse seemed to be hereditary, and in one congenital; in the latter it disappeared as the subject of it became older.

Having placed the seat of the disease in the nervous system, it remains to be decided whether it is seated in the cerebro-spinal system or in the sympathetic system. Holding at one time the view that all mental impressions are received in and that all mental acts proceed from the cerebrum, Dr. Richardson was at first inclined to think that the primary mischief was in the cerebro-spinal system; at the present time he believes that this view is untenable, and that Bichat was right in assigning the great ganglionic system as the seat of all these mental acts which the metaphysician has designated as emotional. As the primary cause of the phenomenon appears to be shock or debility of those nervous centres which are instinctive or emotional, we must of course adopt

the theory that the sympathetic is the seat of the disease. Dr. Richardson has moreover entirely destroyed the functions of the brain by freezing the hemispheres in one of the inferior animals, but although by this method the most complete insensibility was induced there was no satisfactory indication of intermittent action of the heart. This was attributed at the time to incomplete freezing of parts at the base of the brain, but if in this condition the animal is made to inhale chloroform, the intermittent action is at once set up, the ganglionic centres being then involved. It would seem possible that irritation of the pneumogastric might produce intermittence, but experiment has showed that this is not the case.

Intermittence may exist without the knowledge of the person who is the subject of it, especially when the period of intermittency does not cover more than what would be one or at most two normal periods of cardiac contraction and there is a long interval before the return of it; but when the intermittency covers a period equal to five normal strokes, or when it is repeated at shorter intervals several times in the minute, then the patient is painfully, often fearfully, conscious of its presence.

The treatment recommended does not differ from that in general use.

The volume is illustrated by three plates. J. H. H.

ART. XXVI.—*Naval Hygiene.* By JOSEPH WILSON, Surgeon United States Navy: with an appendix. *Moving wounded men on shipboard.* By ALBERT C. GORGAS, Surgeon United States Navy. Reported to the Bureau of Medicine and Surgery. Published by order of the Navy Department. 8vo. pp. 233. Government Printing Office. Washington, 1870.

WITHOUT some knowledge of sanitary laws, the value or propriety of measures which, under equal conditions, medical men commonly recommend for the preservation of health cannot be appreciated. Even the little information of the kind needed for such appreciation is not general among those brave men who command our private and public ships. Through inattention to some circumstance, which to the uninstructed seems insignificant, captains of ships on long voyages "may destroy the health and consequently the efficiency of the ship's company." Official position gives them almost imperial authority. This rightly exercised may, in the opinion of Dr. Wilson, "have great influence in the preservation of human health." But it is very often exercised wrongly through ignorance, and sometimes wantonly through assumption of knowledge, coupled with an almost insurmountable reluctance to adopt any suggestion from a medical associate. These are all adults, very little accustomed to general or special reading, and mostly without that kind of youthful training which enables the full grown man in active life to derive instruction from books. For this class of persons more especially, our author has prepared his work, hoping, no doubt, that by imparting instruction to the superiors, the lowest subordinates in ships might have their chances of health and life increased, while laboring in insalubrious regions. With this object chiefly in view, he presents only such facts and opinions as are common to members of the medical profession. He abstains from the discussion of doctrines and theories, while he endeavours to inculcate in a popular manner such hygienic views as may be easily understood and made practically useful.

The volume cannot be regarded as a systematic treatise on naval hygiene. It is rather a popular essay, which may be freely recommended to those persons to whom it is more especially addressed. In form it is a medical narrative of a cruise of a steamship, which began in the summer of 1860, and closed in the autumn of 1861, through the West Indies, and as far south as Maranham and Surinam, in an unsuccessful pursuit of the notorious cruiser Sumter, Captain Semmes. The outline of the narrative is filled up by hygienic observations upon all the circumstances which influence the health of ships' companies, cruising in

tropical regions, such as the internal condition of the ship as to cleanliness, ventilation, police, discipline, food, clothing, amusements, punishments, &c. &c. The subjects of quarantine, epidemic, endemic, typhus, typhoid and yellow fevers, scorbutus, &c., are treated in separate chapters in a very general way. The experience of the author during more than a quarter of a century in the navy, of which considerably more than half the time was at sea, is interwoven in his work. Besides his experience, Dr. Wilson brings to his self-imposed task, good sense, a habit of careful and close observation, with a natural love of truth and accuracy which renders his work, as far as it goes, valuable, because it is reliable.

The manner and style of the work may be perceived in the following extract from the chapter on "Endemics—malarial fevers."

"We have now pretty well determined the means of avoiding malarial miasm and its consequences; but human nature interposes obstacles. We see men going to nearly certain death: we reason, we explain, we argue; and a man who has never studied the subject suggests his 'I should think,' in opposition to us. The ignorant man's 'I should think' may carry the day against us, and our hygienic precautions go for nothing, except that we can sometimes use the privilege of saving ourselves. We see this in every city over the land; this ignorant 'I should think' controls legislation, and is slaying its myriads. We have the same difficulty in the military service. Expeditions are planned, topographical engineers are consulted about visible obstacles, but no reasonable calculations are made for the diseases which, even in campaigns of hard-fought battles, will destroy three times as many men as all the bullets. The medical man is not consulted till after the mischief is done, the men sick and dying. The surgeon, by his reports, then suggests that the force is being weakened, and may hint at the cause. The subject is discussed by those in authority, who knowing very little about the subject, their discussion is apt to end in something very like the old fashioned 'I should think.' I have always found the most effective way to manage this business, is to set up prejudices in the right direction, by discreet social conversation, and when this produces nothing better than 'I should think,' to follow it up by a report if necessary, the occasion for which may be explained, by stating the propriety of putting certain views formally on record before going into such risks as are contemplated. Such reports should state the case in good terse language, and conclude with a recommendation too pointed to be evaded. If this do not succeed, it is understood that a copy may go with comments to higher and higher authority, till we reach Congress; and the responsibility of a sweeping pestilence is in the right place as near as we can make it so. Sometimes our recommendations may involve alterations in the plans of a department, and then commanding officers feel that they must have such reports before they are safe in acting—even ask for them. The best illustration of the manner in which this has to be done, occurred in China some years ago. We were anchored in the Pearl River, above Canton, opposite Macao pass, and the captain, whom I had pretty well indoctrinated previously, on the subject of rice swamps, sunset boats, and night air, mentioned that he had orders to observe the tides in this part of the river, and hinted at the difficulty of finding a suitable place for a tide staff, the edge of a rice field appearing to be the only available place.

'We are healthy here; cannot we get a post or two down somewhere on a bar, at a distance from the shore?'

'The current is strong; it would be in the way; boats would run against it and tear it up.'

'Perhaps you might get observations enough in the daytime?'

'Hardly; we may not be here long, and the work should not be half done.'

'Leaving all risk of sickness out of the question, how would you arrange it?'

'Place a tide-gauge at the nearest point to the ship, with two men, and relieve them by other two at meal times.'

'A month of such observations would cost us about eight lives.'

'Oh, no.'

'Oh, yes; the first set, six men, down in one week, and others at the same rate; twenty-six in a month, one-third to die—eight and two-thirds; the calcu

lation is a little strong, perhaps, so we will throw off the fraction—eight deaths a month.'

'Well; but we must do something.'

'Men must not spend a night in that swamp; they can pull up to the staff at the right moment, which surely can be anticipated, make their observations, and pull out into the river again, back to the ship if they have time.'

'The current is very strong; hard work, and no rest for the men.'

'Yes; but they had better spend the night, if need be, pulling against the current in the middle of the river, than lounging on shore.'

'Well; we will try it so.'

The plan was tried, and I soon heard that it only required fifteen to twenty minutes to pull ashore on slack water, make the observations and return. There was no difficulty about it. The observations were continued for several months; as long as there was any motive for continuing them, and the results are recorded in the history of the Japan expedition. This little conversation doubtlessly saved several lives, for without it the men would have been spending nights in the rice field before I should have heard anything about the tidal observations."

The contribution of Surgeon A. C. Gorgas to this volume, consists of a description of a cot, to the frame of which is fixed, a double inclined plane, to accommodate the lower extremities of the wounded man, with a strap around the chest, and thus prevent him from sliding down, in case the narrowness of the hatchway requires one end of the cot to be considerably depressed in order to make it descend. The description is illustrated by a diagram. The suggestion of Dr. Gorgas is ingenious and of practical value.

Dr. Wilson says, page 150: "When the wounded are merely to be taken below from the deck, the best arrangement is to have a stout man lift the wounded, by placing one arm back of the shoulders and the other under the knees. Holding him in this way he can carry him either up or down stairs, or a short distance anywhere, with as little jostling or injury as by any available contrivance. If a wounded man be too heavy to be carried in this way, he may be placed in an arm-chair and carried by two or three men. If too severely injured to be handled in a chair, he may be transported in a sick-bay cot, a cord of sufficient length being attached, if it be desired, to lower the cot to the berth-deck."

It is hoped that the publication of this volume by the Navy Department, will encourage members of the naval medical corps to endeavour to add something of scientific value to this department of medical literature. Heretofore the Bureau of Medicine and Surgery has been exclusively a fiscal office, little or no encouragement being given to scientific or professional enterprise.

W. S. W. R.

ART. XXVII.—*Twelfth Report of the Medical Officer of the Privy Council.* Appendix No. 7. *Report by* Dr. SEATON *on so-called "Animal Vaccination" in France, Belgium, and Holland.* London, 1870.

THIS interesting report is based on observations made in the autumn of 1869, during a visit to Paris, Brussels, Rotterdam, and Amsterdam, under official instructions from the Lords of the Privy Council, with the view of investigating the practice of animal vaccination in those cities, and of comparing the results there obtained by this kind of vaccination with the results attending the use of humanized vaccine lymph at the public vaccination stations in England.

With reference to the *transmissibility of cow-pox, unimpaired, from calf to calf*, the statements made to Dr. Seaton were conflicting. M. Lanoix said that since he first brought his calf from Naples in 1864 he had never failed to continue the disease by inoculation. It appears, however, that a certain amount of skill and practice is necessary to obtain a successful result, for M. Warlo-

mont states that now he succeeds perfectly, whereas at the first "his unsuccesses were so numerous that, had he been determined by the results of his first year's work, he could not but have abandoned the practice." Moreover, at Rotterdam, Dr. Bézeth reports that after two or three months the eruption on the calf became much less pronounced, and the results produced on the human subject frequently imperfect and abortive; fresh lymph was obtained, which in every case seemed to undergo a similar deterioration, resulting in these "constantly and almost periodically renewed failures," for which there must be some cause other than the peculiarities of the animals used.

The character and course of the pocks produced on children by animal vaccine lymph were carefully studied in Paris and Rotterdam by the author. The pocks had the same character, ran the same course, and manifested the same varieties as Dr. Seaton was in the habit of seeing produced by the long-humanized lymph used at the public vaccination stations in England.

In order to enable us to arrive at a conclusion as to the *comparative success of vaccination with animal and with humanized vaccine lymph* the author gives the following results of the two modes of practice on children in the Paris hospitals from the 1st of October, 1867, to the 30th of October, 1869, inclusive, and in Rotterdam for the year 1869:—

	Total cases.	Proportion per cent. of inspected cases unsuccessful.
PARIS—		
From heifer to arm	7000	21
From arm to arm	3119	7
ROTTERDAM—		
From heifer to arm	542	12.3
From arm to arm	610	0.65

"So far, then, as evidence at present goes," to quote the words of Dr. Seaton, "it appears quite clear, (1) that the present degree of success attending the practice of animal vaccination is, in comparison with the success attendant on vaccination from arm to arm, very low, and such as to constitute a most serious drawback to its use, supposing that other reasons were deemed sufficiently strong to render its introduction as an alternative proceeding desirable; (2) that much training and experience are indispensable to the attainment of even that degree of success which at present attends it. If practised and most scrupulously careful vaccinators, anxiously endeavouring to make their experiment successful, and neglecting no known precaution for the purpose, find after two years' experience that in vaccinating direct from the heifer to the arm they are obliged in one-eighth of their cases to vaccinate a second time before they can produce any effect, and that in the end very nearly one-fourth of the children who are infected in this way are sent out with that imperfect degree of protection against smallpox which is afforded by only one or two vaccine vesicles, it must, I think, be obvious that by the adoption of such a practice we should be greatly weakening our defences against smallpox. The chance to each individual of a full protection would be very largely diminished, and the danger to the community of spreading smallpox greatly increased by the number of half-protected persons thrown upon it."

With regard to the *preservability of animal vaccine lymph*, Dr. Seaton found that the heifer lymph as compared with ordinary lymph is peculiarly apt to spoil on keeping, and that the tube-preserved lymph cannot be relied on.

Dr. Seaton is favourably known in this country as the author of "A Handbook of Vaccination"—the best book on the subject in our language. His vast knowledge and experience eminently qualified him for the task of investigating the relative merits of vaccination with animal and humanized vaccine lymph, and his conclusions, given above, commend themselves to our entire confidence.

I. M. H.

ART. XXVIII.—*Transactions of State Medical Societies.*

1. *Transactions of the Medical Society of the State of New Jersey*, 1870. 8vo. pp. 214.
2. *Transactions of the Seventeenth Annual Meeting of the Medical Society of the State of North Carolina, held at Wilmington, N. C., May* 25, 1870. 8vo. pp. 138.
3. *Transactions of the Medical Society of the State of Pennsylvania at its Twenty-first Annual Session, held at Philadelphia, June*, 1870. 8vo. pp. 216.
4. *Transactions of the Twenty-fifth Annual Meeting of the Ohio State Medical Society, held at Cleveland, June* 14–16, 1870. 8vo. pp. 300.
5. *Proceedings of the Texas State Medical Association, held in the city of Houston, June* 15, 1870. 8vo. pp. 48.

1. THE leading theme of the "Annual Address" before the *State Society of New Jersey*, by the President, Dr. WILLIAM PIERSON, is the importance of a more thorough study by all physicians of forensic medicine; the necessity of introducing a very full course of instruction in medical jurisprudence in the curriculum of our professional schools, and the rigid examination upon the subject, of their pupils, when they become applicants for the doctorate, or for a license to practise as physicians.

From a *résumé* of the Medical History of the State, presented in the Report of the Standing Committee, we learn that scarlatina and pneumonia were the most prevalent diseases, throughout nearly every portion of the State, during the preceding year; neither of them, however, in a very severe form or of fatal tendency. Pneumonia was much more frequent and prevalent than in former years, and was due, the committee believe, to the continued atmospheric humidity of the winter and spring months. Rubeola was of frequent occurrence, but not more so, perhaps, than in former years. Diphtheria prevailed in a very few localities. The mild and tractable exanthem known as roseola occurred extensively over the State. The committee remark that notwithstanding the diffusion throughout the several counties of the diseases referred to above, they are warranted in reporting the entire State to have been, during the year, healthy to an unusual degree. Wide-spread districts of country, more especially in Burlington and Camden counties, in which, in former years, the several forms of malarial disease prevailed endemically, are now comparatively free from them.

Among the cases of interest recorded in the present volume of Transactions, is one of what the relator terms "misplaced labour-pains." A female, in the eighth month of pregnancy, was seized with intense pain in the right thigh, extending from the great trochanter along the outer and back part of the thigh. Morphia was administered hypodermically and by the mouth, and fomentations to the thigh, but with slight relief. The pains increased in intensity, but were at length suddenly and completely relieved by the birth of a dead fœtus. It was a second confinement, but the patient was not aware she was in labour until the head of the fœtus was passing the vulva.

Dr. MARCY reports a case of cataract of the left eye, of twenty years' standing, in which recently, the opaque lens has been entirely absorbed, after repeated attacks of sclerotitis of the affected eye, occurring within the space of some three months. Shortly afterwards absorption of the upper and outer edge of the cataract was observed, which gradually extended until the opaque lens wholly disappeared. With a magnifying lens the patient can now see distinctly.

2. The subject of the "Annual Address" before the *Medical Society of the State of North Carolina*, at its Session of 1870, by Dr. C. TATE MURPHY, of Sampson County, is the "Heroic Character of the True Physician," and is ably handled.

The address of Dr. C. J. O'HAGAN, the retiring President, is replete with noble sentiments and judicious suggestions bearing upon the standing, the obligations, and the educational requirements of the physician.

Dr. E. B. HAYWOOD relates a successful case of "ligation of the right external iliac artery, for traumatic aneurism of the femoral artery," being, so far as he is aware, the first successful operation of the kind performed in the State. The same gentleman relates the case of a young lady who contracted buccal chancre from the kiss of her affianced lover, who was at the time labouring under secondary syphilis.

A very full and somewhat elaborate account is given by Dr. S. S. SATCHWELL, of the "Medical Topography, and the Prevailing Diseases of New Hanover County, N. C." The following remarks of Dr. S. on the subject of *pulmonary consumption*, are not devoid of interest. "Among our most common diseases, and one that seems to be alarmingly on the increase in this county, is 'pulmonary consumption.' My observations do not corroborate the opinion that either a moist or a malarious climate lessens the liability to its occurrence. Nor have I met with any facts to justify the common opinion that the disease is almost invariably hereditary, or intimately connected with certain climates. This error, conjoined with the popular belief, concurred in too generally by physicians, that the disease is always incurable, is exerting a deleterious influence upon the course of medical science and the claims of humanity. I see cases every year that could not be traced to hereditary taint or climatic influence, but were dependent upon excess in sedentary occupations, too much confinement within doors, over anxiety of mind, bad air of apartments, and the disregard of other hygienic rules, especially the want of proper clothing during our winter months, and of sufficiency of wholesome food the year round." In respect to the prevention, Dr. S. remarks that his experience as a surgeon in the "Confederate army," during the four years of the late war, justifies him in the opinion that most of those who left their homes for service in the army, "with weak lungs, and a tendency to phthisis pulmonalis, and sometimes even with incipient tuberculization, and who escaped the dangers and diseases of the soldier's life, returned home either greatly improved or entirely cured." "The great, the golden rule, then, is to bring up and sustain the general health. The entire physical system should be raised to its highest possible vigour. In the young, proper and well regulated physical culture is the only safeguard against consumption."

The essay by Dr. W. G. THOMAS, on the "Diarrhœa of Children," presents a sound exposition of the causes, nature, and course of the disease, with its treatment, hygienic and medicinal. The complaint is a very common one, and when neglected or mismanaged, it constitutes one of the most serious diseases of early life; even when, in protracted cases, the little patient recovers finally, it is with an impaired organism which predisposes it to an attack, sooner or later, of some fatal malady.

A short report on "Medical Experts," by Dr. E. A. ANDERSON, is the last of the papers contained in the present volume of Transactions. Dr. A. justly maintains that the laws in relation to the conducting of coroner's inquests, and to the position of experts when called upon to testify as witnesses before our judicial tribunals, greatly need revision.

3. The annual address before the *Pennsylvania State Medical Society*, at its session of 1870, by the president, Dr. W. M. WALLACE, though somewhat discursive, is full of interesting and valuable suggestions well expressed.

After some special business reports, a paper by Dr. W. H. GOBRECHT, Professor of Anatomy in the Medical College of Ohio, in evidence of the "Antidotal Effect of Chloroform on Strychnine," and the description of a "Compound Craniotomy Instrument," by Dr. R. L. SIBBET, there follow the "Reports from County Medical Societies." But thirteen of these give any account of the prevailing diseases of the sections of country, the medical history of which during the year preceding their date, they were expected to furnish. Most of these reports are meagre in respect to the materials they present, and deficient in requisite details, and even the least exceptionable, among which we may include the reports from the counties of Beaver, Bradford, Columbia and Montour, Mercer, Montgomery, Northampton, and Schuylkill, describe the sanitary condition of only limited portions of the counties from which they emanate, based upon the observations of only one, or at most two, of the medical prac-

titioners of this or that locality, but without any satisfactory reference to its topography in a medical point of view.

4. The twenty-fifth annual meeting of the *Ohio State Medical Society*, held June, 1870, was opened by an address from the retiring president, Dr. S. M. SMITH, of Columbus, in which he assumes "the less gracious, but, he trusts, the not less profitable office of inquiring into some of the faults and defects in the cultivation of our profession, that retard its progress, leaving to others the more pleasing duty of singing its triumphs, and recording its grand achievements, but claiming an equal sympathy in their pleasing task."

Dr. S. thus briefly catalogues a few of the acknowledged defects and faults in the medical education of the country. Imperfect preliminary education; defective training as office pupils; too short and not sufficiently exclusive time devoted to elementary branches; too early entering upon the study of the practical and applied branches.

Following the ex-president's address is a communication from Dr. P. S. CONNOR, of Cincinnati, on the "Surgical Application of Carbolic Acid," as a deodorizer; as a local irritant; as a caustic; as a destroyer of parasites, animal and vegetable; as a preventive of inflammation and suppuration; and as a true antiseptic, preventing or arresting putrefaction, and protecting from the evil influences of specific miasms. The paper is interesting and instructive.

The next communication treats of the "Climatology and Diseases of Southeast Kansas," by Dr. P. BEEMAN, of Iola, Ks. This is a very full, able and instructive paper. Dr. B. contrasts the States of Ohio and Kansas in a hygienic and medical point of view. "The foregoing hasty and imperfect sketch of southeast Kansas shows a much greater difference between it and Ohio in climate than in diseases. In some respects the climate of Ohio is more desirable than that of southeast Kansas. in others it is less so. If our winds add to the severity of our winters, they modify the heat of our summers. If our winters are a little colder than yours, they are so much drier, and hence more easily borne, so that the advantages and disadvantages are about equally balanced."

The next paper is a "Report of the Committee on Anatomy Law for Ohio." The subject is one that interests every member of the profession throughout our country. We congratulate the physicians of Ohio that at length they have succeeded in obtaining a law from their legislature by which the dissection of a human body is recognized as a lawful act, and for the act itself no punishment can be inflicted; and we trust that they will be able to obtain at an early date such an amendment of the law as shall render it in all its provisions acceptable to the medical profession of the State.

An interesting "Historical Review of Medical Organizations in Ohio," is presented by Dr. E. B. STEVENS, of Cincinnati. It does not admit of analysis.

Dr. E. H. HYATT continues his report on "Hæmatics," the first part of which appeared in the *Transactions* of 1869. The report of the present year is devoted to the consideration of the *catalytic* order of hæmatics, under which head he treats of quinia, mercury, iodine, salines, alkalies, sulphur, arsenic, silver, zinc, lead, copper, antimony, certain acids, colchicum, etc. The entire paper is deserving of a careful perusal. The exposition of some of the leading principles which underlie the action of catalytic hæmatics is, to say the least of it, highly plausible, and, we believe, in strict accordance with well-established therapeutical experience.

The report on "Vaccination," by Dr. WM. B. DAVIS, of Cincinnati, is a very able one. The principal points embraced in it are, first, a brief review of the epidemic of smallpox which prevailed to a greater or less degree during the fall and winter of 1868–69 throughout the United States; second, an inquiry into the alleged degeneration of human vaccine lymph; third, examination of the question of animal vaccination; and fourth, of the question: Is there danger of conveying other constitutional affections than vaccinia by the use of human vaccine lymph?

This fourth and last department of Dr. D.'s report is at the present time one of deep interest, and in reference to it the author's remarks are important, and his collation of the observations upon record bearing upon it very full and conclusive. Over seventy years have elapsed since the introduction of vaccination

by Jenner. The majority of the populations throughout the civilized world have undergone vaccination. If the infection produced by the introduction of vaccine lymph into the human system had a tendency to introduce into it any constitutional disease other than the vaccine, its wide-spread use for almost three-fourths of a century would certainly show, we conclude, a large increase of constitutional maladies. No such increase, however, is proved to have occurred as is shown upon a careful examination of the mortuary record for that period. On the contrary, statistics, according to Farr, Greenhow, and others, prove that scrofula and other constitutional affections have diminished, and that the average duration of human life has been actually extended since the introduction of vaccination. There are certain cutaneous eruptions which, to use the words of the report, may follow vaccination, just as they may teething or any other slight constitutional disturbance. Niemeyer, in his "Practical Medicine," says: "We know that blisters and other irritants of the skin not only induce inflammation at the point of application, but also increase the predisposition to certain cutaneous eruptions. Children who previously have never suffered from any exanthem often suffer for months with moist eczema of the face after having their ears pierced, as well as after vaccination."

"Vaccine lymph," very properly remarks Dr. Davis, "when long exposed to air and moisture will undergo the same putrefactive changes that any other animal matter does when similarly exposed, and if it be then inserted in the arm of a subject it will produce the same effects as any other putrid matter would. When erysipelas or pyæmia follows the use of decomposed lymph, it is not because vaccination has been performed, but because the vaccinator has been criminally careless or criminally ignorant.

"The advocates of transmission, however, would seem to have abandoned their claim for the transmission, by vaccination, of any constitutional disease but syphilis; upon this one disease they rally their full strength."

To disprove this Dr. D. adduces a mass of apparently the most conclusive evidence.

In the fourth number of the *American Journal of Syphilography and Dermatology;* New York, October, 1870, will be found a paper, by Dr. Frank P. Foster, in which a brief review is taken of the leading cases on record upon which the doctrine of *vaccino-syphilitic inoculation* is based, and shows by such review that no one of the cases presents any positive evidence in proof of syphilis having been communicated to a non-syphilitic patient by inoculation with lymph taken from a typically developed eight-day vaccine vesicle.

The description of the disease and the plan of treatment, as given by Dr. O. G. Seldon, in his report on "Pneumonia," are applicable in a greater extent to the disease as it occurs in the inhabitants of malarious regions, than they do to the acute uncomplicated cases of pneumonia incident to robust subjects in more healthy localities. In a practice of over half a century in the city of Philadelphia, in the vast majority of the cases of pneumonia that have fallen under our notice we have invariably found that, at the onset of the attack, bleeding from the arm or by scarified cups was strongly indicated, while its employment was, in every instance, productive of prompt and unmistakable benefit.

Dr. J. R. Black points out, in the next communication, with great force and truthfulness, "the importance of a Concerted Account being obtained of the Prevailing Diseases and their Management throughout the State; and it being," concludes Dr. B., "within the power of collective bodies of medical observers, properly organized, to accomplish in this respect what individuals cannot, it should be to the obtainment of the objects referred to that the energies of such organizations are directed, and that, in this manner they may render themselves efficient promoters of scientific and practical medicine.

The paper next in order is a report by Dr. W. J. Conklin, Assistant Physician of Southern Ohio Lunatic Asylum, at Dayton, on "the Temperature of Certain Nervous Diseases," or more strictly "the value of the thermometer" in the study of the nature and symptomatology of those affections. The subject is an interesting one, and ably discussed by Dr. C. A satisfactory analysis of the paper would occupy a greater space than could conveniently be devoted to it.

On "Cantharism," by Dr. ALEX. MCBRIDE. The term *cantharism* is applied by the doctor to "an induced physiological condition not hitherto described;" it is, he remarks, a peculiar and salutary one, which is established in the human economy by a few large doses of tincture of cantharides, ʒj to ʒv, or a long continuance of small doses ♏ xx to xxx. The cases in which he has had most frequent opportunity to notice the beneficial effect of the remedy have been "the dangerous extremity of disease—where death was imminent from congestion, engorgement, blood stasis, or collapse; such as cases of typhoid pneumonia, congestive intermittent, congestive stage of continued and malarial fevers, cholera, choloroid, and the like." "In some of these cases," he states, "the induction of the state of cantharism produces a very interesting effect. The congestion, flux, or chill is usually arrested or checked by about two to three drachms of the medicine in one to two hours' time. The change begins to be observable usually in fifteen to thirty minutes; sometimes not for an hour or more; the exalted frequency of pulse abates, its thready, gaseous, and evanescent character is changed for one of a definite form under the finger; the livid features assume their natural hue; warmth returns to the extremities; the moist rattling of the lungs abates; fear and despair vanish, and hope is restored. In an hour or more the medicine is repeated to confirm the favourable impression, and so on in a less dose from time to time, according to indications." The patient generally recovers, Dr. McB. tells us, excepting in cases where vital parts are already destroyed. Even in these hopeless cases there will often occur a decided amelioration—life is generally prolonged for many hours if not for days.

The final report comprises the "Obituaries" of those members whose deaths occurred during the preceding year; they were four in number. The report is compiled by Dr. E. B. STEVENS, of Cincinnati.

5. The annual session of the *Texas State Medical Association*, held June, 1870, was opened by an address from its president, Dr. T. J. HEARD, in which he advances his views as to the advantages that would accrue from the establishment by the General Government of a National School of Medicine. Such an institution is manifestly altogether impracticable, and even if it could be carried out, the result would be to convert medical teaching into a political machine, and instead of elevating every department of scientific medicine it would have the opposite result, while it would most certainly tend to degrade the members of our profession into time serving politicians.

A very good sketch of the "Science and Progress of Medicine" is furnished by S. M. WELCH.

Professor GREENSVILLE DOWELL relates a case of "Rupia, accompanied by Gout." The occurrence of rupia in gouty subjects does not appear to us, judging from our own experience, to be so unfrequent an occurrence as Dr. D. would seem to imply.

The printed proceedings before us close with a "Biographical Sketch of Dr. Enos T. Bonney, deceased," by Dr. W. J. COOKE.

From the zeal by which we find the leading physicians of Texas to be animated in the promotion of medical organization throughout the State, we anticipate in the future a series of valuable contributions towards the improvement and enlargement of medical knowledge, and its perfection in a practical point of view. D. F. C.

ART. XXIX.—*Aiken; or, Climatic Cure.* By AMORY COFFIN, M. D., and W. H. GEDDINGS, M. D. 12mo. pp. 58. Charleston, S. C., 1870.

THE leading object of this very neatly gotten up pamphlet, is to show the advantages of the little town of Aiken, South Carolina, as a residence especially adapted for persons predisposed to, or already labouring under, pulmonary tuberculosis. The first portion of the treatise of Drs. Coffin and Geddings is

devoted to a very concise, but at the same time highly interesting consideration of the influence of climate on phthisis pulmonalis, in reference to its production, on the one hand, and on the other, to its amelioration and its cure. In directing, more especially, their remarks to the relation between climate and pulmonary tuberculosis, the authors of the present pamphlet remark, that they do not wish to be understood as confining its good effects to that disease alone; it is equally beneficial in many others, either pulmonary in their seat, or asthenic in their character, such as chronic bronchitis, or pneumonia, catarrhs of various kinds, and slow convalescence from acute diseases—those convalescences which are so imperfect as never to obliterate the stamp of the original disease, if left to accomplish themselves under unfavourable circumstances. They have dwelt more on consumption because of its great and rapidly increasing prevalence, the great interest consequently attached to it, and their convictions that at Aiken is to be found a "combination of circumstances constituting a climate calculated to influence beneficially a large majority of consumptive subjects."

Drs. Coffin and Geddings lay down as the climatic conditions and agencies, which an ample series of observations has shown to be the least liable to concur towards the origination of consumption, and the best adapted to arrest its progress when already present, as follows: "1st. *Dryness* is the *first* attribute of a good climate for consumptives. 2d. *Elevated positions* are much to be preferred to low levels. 3d. *Fresh* and *pure air* is indispensable to the consumptive; consequently that climate is best in which the patient is able without discomfort to spend the greater portion of his time in the open air." Drs. C. and G. endeavour to show that the climate of Aiken fulfils all these requisites; that it is free from all injurious agencies, while at the same time it possesses in a high degree those qualities which are proved to be beneficial. It is colder in Aiken than in Charleston or Savannah, where the temperature is greatly mitigated by the proximity of the Gulf Stream and the ocean. It enjoys the dryness of Minnesota, without its excessive cold, and a climate comparatively mild without the enfeebling moisture of that of Cuba and Florida. The success of our authors in the establishment of the position thus assumed must depend upon the result of future accumulated experience.

In conclusion, we would remark that the chapter on the curability of consumption (arrest of the continued and destructive development and march of pulmonary tuberculosis) is replete with interest, and at the same time highly suggestive.

D. F. C.

Art. XXX.—*Peripheric Linear Operations for Cataract.*

1. *Des Nouveaux Procédés Operatoires de la Cataracte.* Par L. Wecker. 8vo. pp. 19. Paris: Adrien Delahaye, 1868.
2. *An Improved Method of Extraction of Cataract, with Results of* 107 *Operations.* By J. R. Wolfe, M. D. 8vo. pp. 71. London: John Churchill & Sons, 1868.
3. *Report and Remarks on a Third Series of One Hundred Cases of Cataract Extraction by the Peripheric Linear Method.* By H. Knapp, M.D. 8vo. pp. 29. New York: Wm. Wood & Co., 1869.
4. *On Extraction of Cataract by Von Graefe's Peripheral Linear Section.* By Henry Wilson, F.R.C.S. 8vo. pp. 22. Dublin, 1870.

The above-mentioned pamphlets by no means comprise all the noteworthy essays upon the extraction of cataract which have appeared within the last few years; their perusal is, however, interesting as showing to a considerable extent the general drift of professional sentiment on this subject in the various countries in which they have been published. Dr. Wolfe and Mr. Wilson both commence their essays with a short history of the operation of extraction. The former speaks doubtingly of the instances sometimes quoted as to the extraction of cataract by the ancients, while the latter asserts that they have but little foundation and rest upon a faulty translation of the originals. Mr.

Wilson quotes Prof. Hirsch, of Berlin, as giving his authority to this view. They ascribe to Daviel (1745) the credit of having first accurately described the flap extraction of cataract, and having introduced it as a method of operating.

Mr. Wilson, in the course of his essay, makes some interesting remarks on the form of the orbit in the native Celtic population of Ireland, and shows how their small and deep-set eyes, with prominent overhanging eyebrows, complicate and render more difficult the extraction of cataract whether it be by the flap or linear method. In the average European head a line drawn from the middle of the eyebrow to the lower orbital margin forms a tangent to the cornea when covered by the closed lid. Let us now contrast this with Mr. Wilson's measurements. "If a line be drawn from the eyebrow to the lower orbital margin and another from this line to the cornea, the latter one often measures 6'''—in one case of flap extraction it measured as much as one English inch; in this case the space from angle to angle measured not quite $\frac{3}{4}$'', and the natural opening of the eyelids $\frac{1}{4}$''." If such measurements as our author alleges "though not common are yet not exceptional," we can readily see that the mechanical conditions of the operation are widely different from those with which most of us have become familiar, and can readily appreciate the cause of the small size of Sir W. Wilde's flap extraction knives, which Mr. Wilson informs us measure only 9''' on the cutting edge, while those of most continental makers average from 13'''–15'''.

Dr. Wolfe, in his brochure, gives us a *résumé* of the various methods of extraction most in vogue during late years, and concludes by urging the profession to adopt his method of extraction by "iridectomy and small corneo-conjunctival flap." He performs the iridectomy about six weeks prior to the extraction, except in cases of traumatic cataract with dislocated lens, in which case they are executed at the same sitting.

Except the small size of the flap there is but little new in the operation, and indeed he does not make claims to great originality of method. Preliminary iridectomy is an idea long familiar to ophthalmic surgeons and extensively practised by some. Dr. W. himself shows that Mooren had used it extensively (1862) previous to extraction, and credits Desmarres with having introduced the conjunctival flap (1849). Dr. Wolfe gives us a tabular statement of the results of 107 extractions by his method. Of this series 94 cases or 87.84 per cent. are represented as healing well with *perfect vision*. Certainly a very satisfactory result, but one, we think, which would carry more weight with his readers if he had informed them what degree of acuity of vision he considers "perfect vision" after a cataract operation. We would respectfully suggest that any future series he may publish be accompanied by a table of acuity of vision, determined by Snellen's tables in the usual way. Any method which necessitates two separate operations appears to us disadvantageous, especially in case of nervous patients and in those coming from a distance.

A considerable portion of all these reports is occupied with the discussion of Graefe's peripheric linear method of extraction; its advantages as compared with the old flap extractions; its complications, results, and sequelæ.

While all admit that the old method of extraction by corneal flap gives, when successful, most admirable results—results perhaps more perfect than those attained by any other method, nevertheless the very considerable proportion of failures which follow its use, even by the most skilful operators, has of late years led ophthalmic surgeons to see in what method it might be advantageously modified or substituted. It is claimed that the peripheric linear method offers the following advantages:—

1. More accurate adaptation of the edges of the wound and a more rapid union, necessitating a much less time of absolute and irksome quiet in bed.

2. That the more peripheral nature of the wound—a portion of it falling entirely within the sclerotic—the rapid union and nominal size of the flap cause a much less tendency to suppuration of the cornea.

3. That owing to the situation of the wound the lens requires but a slight degree of rotation on its axis to enable it to glide out of the wound, and for this reason and the large iridectomy there is less bruising of the iris, and consequently fewer inflammations of it and of the uveal tract.

4. That the total duration of the treatment is notably abridged, and that the patient can return to his home or to his ordinary occupations at an earlier period.

A glance at the statistics published by some of the most distinguished ophthalmic surgeons, embodying the results of an extensive practice of both methods of extraction, will enable us better to decide as to the justice of these claims. Wecker, in his pamphlet above quoted, gives us the result of his own experience and that "of his contemporaries most worthy of confidence," with the flap extraction: 10 per cent. of immediate total loss, 5 per cent. who are practically blind at last, 10 per cent. who after a secondary operation are able to go about by themselves, thus leaving 75 per cent. only of immediate good result.

Graefe,[1] in summing up the results of 1500 cases of flap extraction performed by himself, gives only 65 per cent. of immediate good results ($V=\frac{1}{2}$ and over —in patients beyond 75 years $V=\frac{1}{8}$ is counted a good result), 15 per cent. give good results after a secondary operation, and of the 20 remaining cases, 6 retained enough vision to go about alone ($V=\frac{1}{30}-\frac{1}{50}$), 6 had still a less degree of vision, and 6 or 8 became absolutely blind (*i. e.*, no perception of light).

In a later publication,[2] however, the same author gives 84 per cent. of good results, 11 per cent. half successes, and 5 per cent. of absolute blindness, and remarks that in his private practice the results are even more favourable, viz., 91 per cent. good results, 6 per cent. half successes, and 3 per cent. of absolute loss.

Let us now turn to the results of the peripheric linear method. The author of it himself has not given us so extended and full an analytical report of his cases as would be desirable; as Knapp remarks in his report, "We are intimate friends of Graefe's operating room, but only casual visitors of his hospital wards." General data he has however furnished us, and probably had he lived we should have had at a later period his complete results. We must endeavour, therefore, to content ourselves with what he has given us. His first series of cases (*Archiv für Ophth.* 1865) show 62 good results and 7 half successes out of a total of 69 cases—no absolute loss.

In a later paper[3] he gives as result of 500 operations, 90 per cent. as immediate good results (V between $\frac{1}{4}$ and $\frac{2}{3}$) and 10 per cent. of half successes and failures, most of which could be benefited by secondary operations, giving a grand total of 94 per cent. of successes (V more than $\frac{1}{4}$) after the appropriate secondary operations had been performed, a result much surpassing that which (as we have seen) the flap operation yielded in his hands.

Prof. Becker[4] gives the statistics of 217 cases of peripheric linear extraction, performed at Professor Arlt's Clinic (150 operated on by Arlt in person) which show 81 per cent. immediate good results, 10 per cent. in which a secondary operation was necessary, 5 per cent. of loss (2 per cent. absolute blindness and 3 per cent. mere perception of light) 10½ per cent. with $V < \frac{1}{10}$. Iritis was the most common complication (in 13 per cent.), and irido-choroiditis the most fatal, occurring in 4 per cent. of cases. In a less number of cases Wecker gives similar statistics.

Knapp's report on his last 100 cases is exceedingly interesting from the particularity with which he has described the accidents most likely to occur after and during the operation. His results are more favourable than in either of his formerly published series of cases, and he states that he has adhered more closely to Graefe's directions in performing it. At the end of his paper he gives a table of acuity of vision in all cases, and sums up with the following statistics: 82 per cent. primary good results ($V > \frac{1}{10}$), 3 per cent. losses, and 15 per cent. partial successes. Secondary operations converted many of the imperfect results into successes, giving a total of 91 per cent. of good results. Iritis was

[1] Zehender—Klinische Monatsblätter, 1863. p. 146.
[2] Archiv. für Opth. vol. ii. part 3, p. 7, 1865.
[3] Loc. cit. vol. xii. part 1, p. 151.
[4] Zehender Klin. Monatsbl. f. Augenheilkunde, vol. v. p. 279, 1867.

the most frequent complication occurring in 7 per cent. of the cases, and there was 6 per cent. of after-hemorrhage, 4 per cent. of capsulitis.

As regards duration of stay in hospital, Prof. Arlt gives an average of 17½ days by this method against 28–30 days for flap extraction. Mr. Wilson gives an average of 21 days for the linear and 45 days for the flap. Knapp gives 14$\frac{7}{10}$ days as the average stay of his patients, and Graefe even a less period.

A consideration of such statistics open up many interesting inquiries as to minute differences of method, the period of operating, the degree of ripeness of the cataract, the more or less peripheral nature of the wound, the frequency of prolapse of the vitreous, the period at which secondary operations may be most advantageously performed, the treatment of subsequent inflammations, etc. But we have already exceeded our proposed limits, and must rest satisfied with having presented to the reader, for comparison and study, some of the more noteworthy and reliable statistics referring to the extraction of cataract by the "peripheric linear method." W. F. N.

ART. XXXI.—*Pathological Psychology.*

1. *Medical Opinion of* CHARLES A. LEE, M. D., *in the Matter of Carlton Gates, deceased.* 8vo. pp. 30. New York, 1870.
2. *Voluntary Patients in Asylums.* By STANLEY HAYNES, M. D. 8vo. pp. 16. London, 1870.
3. *Nature and Treatment of Hereditary Disease.* By J. M. WINN, M. D. 8vo. pp. 31. London, 1870.

1. THESE pamphlets are connected only by the common tie of belonging to the same department of science—pathological psychology—and, though the subjects which they treat are not very closely allied, yet they all illustrate the progress of opinion in certain directions. That of Dr. Lee, whose name is not unfamiliar to the medical readers of this generation, is founded on a will case in which the testator was found to be incompetent, and consists of an elaborate exposition of the grounds on which the doctor rested his opinion. It is an admirable *résumé* of the doctrine of insanity as applicable to the class of cases in question, but maintains those views which are as yet much regarded as more remarkable for novelty than soundness. The doctor declares that much of the insanity in the world is accompanied by no delusion, and has the boldness to say that moral insanity is not a mere crotchet, the fruit of a loose and superficial observation, but an actual and serious fact. In adverting to the common notion that on subjects remote from his delusions, many a patient is as reliable as ever, he questions its correctness, and believes that a close and protracted observation will generally show some moral or intellectual impairment over and above the specific delusions. And we may say, in passing, for it needs to be said often, that testamentary capacity depends not only on the strength of the memory whereby the persons and things chiefly concerned are brought to mind, but on a soundness of judgment sufficient to estimate conditions and circumstances, and discriminate rightly between the claims of one and another on the testator's bounty. The tendency of all insanity is to weaken the judgment, and the fact should be taken into the account as well in cases of the most circumscribed delusions as of general mania or dementia.

We wish we could indorse the author's statement that partial and total moral insanity have been recognized by the courts of England and of the United States as elements of incapacity in criminal and civil acts. Such has been the fact in a few instances in this country, though somewhat indirectly; but the English courts have shown it no countenance whatever. In another statement of Dr. Lee we are obliged to disagree with him. If, as he says, "statistics prove that intemperance is the most frequent cause of insanity in the United States," it would be well, before adopting this conclusion, to prove the correctness of the statistics. The idea of proving anything by our present statistics of insanity will be received with a smile by many who happen to know how they have been obtained.

Dr. L. closes his pamphlet with a notice of the American decisions connected with this subject, which is a very fitting supplement to the views he has put forth.

We regret that one who writes so well as Dr. Lee, and who has taken the pains to print a paper destined to have a permanent value, should have suffered such gross typographical errors to escape him. Who would ever suppose that "Mandeley" really means Maudsley. Prichard is spelled with a t; Addams is changed into Addoms and Addom's; Aeberton, we suppose, is meant for Heberton, and so on.

2. Dr. Haynes advocates such a change of the law as will allow persons who wish it to place themselves in the hospitals for the insane, for it seems that these institutions are now absolutely prohibited from receiving any without a medical certificate of insanity. It is well known that persons who feel the approach of an attack of insanity, are often disposed to avail themselves of the care and protection afforded by hospitals, and it is a matter of pure humanity to interpose no obstacles to their purpose. The disease at this period is not sufficiently demonstrative to warrant a physician to give a certificate, and consequently the patient must go voluntarily, if he goes at all. But personal liberty is too strictly guarded for any such measure as that. It would lead to that wrongful confinement which is popularly associated in the average English mind with the idea of hospitals and asylums for the insane. No matter how strongly it may be called for by the voice of humanity and science, the newspaper-press is ever ready to sound an alarm loud enough to drown all other voices. We are somewhat press-ridden here, but we have hardly come to that, though we soon shall at our present rate of progress. Nothing better illustrates the superlative absurdity of the current notions on this subject than the existence of a law on the statute book which prevents a man from resorting to the only efficient measure for preserving his health and saving himself and family from probable danger. Had such a thing been observed among a semi-civilized people, it would have excited a smile of derision, and been urged as a good reason for missionary efforts among them.

3. Dr. Winn's attention has been much directed to the hereditary character of disease, but he records nothing very definitely as the result of his own observations, nor does he seem to be very well aware of what others have seen or written. From the more common facts of hereditary transmission of disease he is inclined to infer the existence of a law with a wider sweep than has ever been attributed to it before—no less than a "correlation of morbific influences." This is important if true, as the newspapers say, but unhappily the proof he offers is too meagre to be worthy of serious attention. Indeed he relies chiefly on that doctrine of the correlation of physical forces which has been received with so much favour by the philosophers of our day. We take the opportunity to say, however, that in our humble opinion no field of philosophical inquiry is more likely to reward the student than that which embraces the hereditary transmission of disease. If he is well-grounded in the principles of natural history, willing to devote himself to the observation of disease under various conditions, and skilful in eliciting truth from discordant testimony, we can scarcely doubt of his success. I. R.

ART. XXXII.—*The Raising and Education of Abandoned Children in Europe; with Statistics and General Remarks on the Subject.* By ABRAHAM JACOBI, M. D., Member of the Medical Boards of the Infant's Hospital, Randall's Island, and of the Nursery and Child's Hospital, New York; Professor of Diseases of Children, College of Physicians and Surgeons, New York, etc. 8vo. pp. 43. New York, 1870.

THE substance of this interesting pamphlet was presented originally in the form of a report to the Medical Board of the Infant's Hospital, Randall's Island.

The subject of which it treats is of deep interest, alike to the legislator, the philanthropist, and the physician. While the first two classes adopt the principle that society is responsible for the life and person of every individual within its bounds, and is hence bound to take charge of and provide for the well-being, physical, moral, and intellectual, of all its children, especially of those who are abandoned by their natural nourishers and protectors, and who are unable to take care of themselves physically or mentally; while the physician on his part feels it to be at once his duty no less than his privilege to inquire into the causes of the heavy mortality among the inmates of nearly all the institutions which in Europe are devoted to the reception and rearing of foundlings and orphans generally; and the causes of this mortality being determined, to indicate, if happily they are remediable, the means adapted for their removal, or, at least, for the avoidance of their deleterious results.

It will, we think, be no very difficult task to arrive at the nature of the causes, both remote and immediate, productive of the proportionally large mortality which takes place among the inmates of foundling and orphan asylums, and the nurseries and hospitals for children generally. How far these causes may be effectually guarded against or removed, and by what means, is a proper subject for investigation by the physician; a subject which, if properly and fully inquired into, is adapted to yield highly profitable results, and its being entered upon is loudly called for now, especially, when the institutions referred to are being rapidly multiplied in all our larger cities. D. F. C.

ART. XXXIII.—*Annual Report of the Surgeon-General, United States Army.* 1870. Washington, 8vo. pp. 10.

THIS report furnishes us with the interesting information that "the printing of the medical volume of the first part of the Medical and Surgical History of the War is near completion. This volume embraces the statistical tables representing the sickness, mortality, and discharges from service on Surgeon's Certificate of Disability, of *white* and *coloured troops* during the war, and will be a work of nearly 750 pages, quarto. * * With this volume will be bound," it is further stated, "the appendix to the first part of the Medical and Surgical History of the War, containing the reports of Medical Directors, and other appended documents—about 400 pages.

"The whole of the manuscript for the surgical volume of the first part of the Medical and Surgical History of the War, authorized by the act of Congress, approved March 3, 1869, is now prepared, and several of the more important subjects that would belong to the second volume, as, for example, the tabular statements, discussions, histories of typical cases (with illustrative wood-cuts and lithographs) of 29,572 cases of amputations, and 4775 excisions, are nearly perfected. Besides this, the surgical statistics of the army for the five years succeeding the war have been consolidated and arranged for publication."

We take pleasure in adding our testimony to that furnished by the highest military surgeons in various portions of the world, as to the value of the work done by the competent and industrious officers in the Surgeon-general's office to utilize the vast surgical and medical experience gained during our late war; and we believe that we represent the sentiments of the profession, not only in this country but elsewhere, when we express the hope that Congress will make a liberal appropriation to complete the Medical and Surgical History of the War.

The portions already published have obtained the most flattering recognition, both at home and abroad, and gained for American medical science the highest credit. The work, when completed, will constitute a fitting monument to the noble zeal, indefatigable labour, and unselfish devotion of the medical staff of the army, and will be an honour to the government, that liberally fosters these statistical and scientific researches, which have for their direct object so noble an aim—the alleviation of human suffering.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *On the Internal Architecture of the Bones and its Significance as to the Question of the Growth of Bones.*—By Dr. Julius Wolff, of Berlin. This is a most interesting and suggestive paper, and is illustrated by two very fine photographic plates, which add considerably to its value, as the photographs are taken from actual sections of bone. By the study of these photographs one is readily convinced that the lamellæ of the spongy part of the femur, which is taken as the most simple illustration, are not arranged in an arbitrary manner without definite direction, but pass in distinct curves, which are seen to have special directions in different regions. Photographs are given of no less than eight different heads of femurs, and these definite lines of direction are seen to be the same in all. The first part of the paper is intended to show that these directions represent distinct lines of force, and that the spongy bone of the head and neck of the femur is constructed according to Culmann's theory of graphical statics. The author constructed a crane of the same general form as the femur, and asked a pupil of Culmann to fill in the lines of traction and pressure, according to the theory of the latter. The result is given in a plate, and these lines are seen to correspond exactly with the lines formed by the lamellæ of the bone, and our author pertinently remarks that the fact of the femur being constructed in this way, is a striking confirmation of Culmann's theory. In the figure of the crane referred to it is seen that, while in the parts corresponding to the head and neck the lines follow the directions of the lamellæ of the spongy bone, in the parts corresponding with the shaft they fall out towards the periphery. The inference drawn from this and other facts is, that the lamellæ which form the compact bone of the shaft are the continuation of the lamellæ of the spongy portion, and that therefore the shaft, as well as the head and neck, is constructed on this principle. A few photographs are given of other bones, in order to show that the theory holds good for all the bones of the body, although the femur offers the simplest mathematical problem.

The application of these facts to the growth of bones threatens to revolutionize the prevailing ideas on this subject, and our author devotes the second part of his paper to the consideration of these relations. By his photographs he shows that a femur, of whatever size and age, is constructed on exactly the same mathematical principles; the directions of the lamellæ are the same in all. A larger femur is, therefore, merely an enlarged copy in every respect of a smaller one. Now the prevailing belief as to the growth of bone is, that it proceeds by apposition of new bone externally and absorption internally, the deposited bone remaining in *statu quo* till it comes to the internal surface, and

is in turn absorbed. On this theory the lamellæ of the spongy portion which lay externally would come through time to be placed internally, and those which were superior would gradually, as new bone was formed, become inferior. But it is obvious that the lamellæ cannot be dislocated in this way without disturbing the whole architectural arrangement of the bone, and the photographs show that in growing bone no such dislocation takes place. The growth of bone then, like that of muscle and all other tissues of the body, is by interstitial expansion; each lamella of the spongy or compact portion simply increases in size, and the larger bone becomes in its utmost details a copy of the smaller. The fact thus theoretically established he has tested by experiment, and has found that a long bone does not increase in length any faster at the ossifying margin than in the fully ossified portion of the shaft. He considers that our ideas of increase in length going on mainly at the ossifying margin arise very much from confusing growth with ossification. In other experiments he has found that when a wire is fixed in connection with the bones of the skull, so that it will not recede before the growing cranial bones, the portion of bone in contact with it is bent in towards the brain, and microscopic sections show that the indentation is not caused by absorption and deposition, but that the lamellæ are continuous from the straight into the indented portion. The importance of the subject has induced us to give a somewhat lengthy abstract, but the paper must be read to appreciate its importance.—*Glasgow Med. Journal*, November, 1870, from *Virchow's Archives* for June, 1870.

2. *Influence of Diet on the Composition of Bone.*—M. PAPILLON has communicated to the French Academy some experiments made on pigeons and rats. These animals were fed daily for months with food containing small quantities of the phosphates of strontia, of magnesia, and alumina. No visible effect was produced in their health. On analysis of the bones these substances were found in them. If these substances can be made to enter into the composition of bones by an appropriate diet, there seems some grounds for hoping that we may be able to modify animal tissues by means of medicine, and this is a subject well worthy of further experimentation.

3. *Muscular Fibres of the Diaphragm in the Fœtus and Newborn Infant.*—In a paper by Dr. CAZALIS, which appeared in the third volume, 1870, of the *Archiv de Physiol.*, it is stated that, in the human subject during the last months of utero-gestation, and immediately after birth, the muscular portion of the diaphragm exhibits greater vascularity and a more advanced development in the several histological elements than do the muscles of the trunk or limbs. This single fact shows that, the respiration at and for some time subsequent to birth is almost entirely abdominal, and that the diaphragm is the sole agent by which it is kept up and regulated. In the lower animals, the porpoise for example, there is observed a more advanced development and a greater activity of the muscles of the extremities than in the human subject, while the early development of those of the diaphragm is by no means so apparent.—*Centralblatt. f. d. Med. Wisschftn.*, Sept. 8, 1870. D. F. C.

4. *Some Principles in Respiratory Mechanics.*—Mr. D. C. MCVAIL presents the following conclusions to which he has arrived: 1. But for the angular position at which the costal cartilages articulate with their respective ribs, from the third or fourth downwards, thoracic movement would have been altogether impossible, as the natural curvature of the spine is a mechanical difficulty in the way of inspiration, which is only obviated by lengthening of the lower conjoined ribs and cartilages, by each angle of union becoming less and less acute as the chest-wall is raised. 2. Thoracic enlargement does not, as hitherto supposed, reach its maximum when the ribs attain to the angle of ninety degrees to the spinal column. Were this true, the range of thoracic enlargement would be extremely limited, as some of the ribs, even at complete expiration, are almost, if not altogether, at right angles to the spine already. The chest does not reach its greatest volume until the lines of costal action are at right angles to the sternum; a line of costal action being—as regards

the sternum—a straight line drawn from the fulcrum of the rib to the insertion of its cartilage in the sternum. Those lines, in the case of the lower ribs, do not coincide in their respective ribs, and the angular distance between each rib and its line of action increases from the fourth rib downwards. 3. The diagram proving the above also explained the cause of the reverse action of the lower end of the sternum in far advanced cases of emphysema. 4. Reasoning from mechanical theory, it might at first sight be expected that in inspiration the upward movement of the sternum ought to exceed considerably its movement forwards; but experiment has shown that such is not the case, the forward movement being much greater than the upward. The method of conducting those experiments was explained, and the causes of the apparent discordance given.—*Brit. Med. Journ.*, Oct. 1, 1870.

5. *Experiments on Digestion.*—The chemical and physiological conditions present in the alimentary canal are so complex in their nature, that every painstaking inquirer, notwithstanding the frequency with which the ground has been turned, seems to make fresh discoveries, or at least to open up new paths of research. The most recent investigations that have been made are those of Dr. Paschutin, who has had the resources of the excellent laboratory of Professor Setschenow, of St. Petersburg, at his disposal. His researches have chiefly been directed to the determination of the digestive powers of infusions of the mucous membrane of the intestine for albuminous compounds, and on the nature of the ferment converting starch and cane sugar into grape sugar. He states that aqueous infusions of the mucous membrane of the small or large intestine of the dog, when maintained at a temperature of about 100° F., undergo self-digestion, and in the course of three or four hours throw down voluminous precipitates of albumen that undergo no further change till putrefaction sets in, the liquid at the same time altering its reaction from alkaline to acid, and the material, whatever it may be, that is able to reduce oxide of copper, and which is always present in the fresh infusion, disappearing. The infusion of the mucous membrane appears to have *no* power of converting albuminous compounds into peptones, and only a slight power of emulsifying fats. He also found that the secretion of the small intestine obtained by Thiry's method was equally inoperative as regards albumen and fats. The infusion of the mucous membrane of the small intestine, however, can convert starch into sugar. It is curious to notice that this power is possessed also by the mucous membrane of the trachea, urinary bladder, and, though to a less marked extent, by that of the gall-bladder, cæcum, large intestine, stomach, and rectum. None of these, however, can, like the infusion of the mucous membrane of the small intestine, convert cane sugar into grape sugar. This last power he has found to be possessed by the dog, pig, rat, mouse, and rabbit, but not by the membranes of the sheep and cow.

Further investigations enabled him to demonstrate that the infusions of those membranes that can convert starch into sugar and cane sugar into grape, contain *two* distinct ferments, the mode of separation of which would be too long to insert here. The infusion of the muscular layers of the small intestine furnishes the chief part of the ferment capable of converting starch into sugar. A temperature exceeding 104° F. seriously interferes with the action of the ferments. With regard to the saliva, he found that the temperature at which its peculiar ferment, when diluted with ten or twelve parts of water, acts upon cooked starch most intensely is from 100° to 106° F. It is more difficult to determine the temperature at which saliva exercises its greatest activity on uncooked starch; but it is certainly higher than the latter degree. When the ferment is weak it is probably about 144° F.; when concentrated, about 149° F. Dr. Paschutin shows that the admission of air at ordinary temperatures to starch paste speedily causes the formation of a material that is capable of acting as a ferment, and is capable of reducing oxide of copper. Such modified starch-paste can act as a ferment to fresh paste, but the power is destroyed by boiling. —*Lancet*, Oct. 1, 1870.

6. *Physiology of the Pancreatic Secretion.*—M. BERNSTEIN gives, in the just published *Arbeiten aus der Physiologischen Anstatt zu Leipzig*, the results of a series of investigations, conducted under the superintendence of C. Ludwig, on the physiology of the pancreatic secretion. In opposition to Claude Bernard, he thinks that perfectly natural and healthy secretion can be obtained from dogs in which a permanent fistula of one of the ducts has been established; since he found that the fluid thus obtained was able to convert starch into sugar, to emulsify fat, and to digest albumen, or, in other words, to convert it into peptone. He finds, in accordance with previous observers, that the activity of the gland undergoes variations standing in direct relation to the ingestion of food, the quantity of the secretion discharged increasing immediately after food has been taken, and attaining its maximum about two or three hours after a full meal; it then diminishes to the 5–7th hour, when it again augments slightly, and finally falls to zero at the 15th hour. His most important investigation, however, was in reference to the influence of the nerves on the secretion. He found that the production of nausea greatly diminished the flow of the secretion, whilst actual vomiting almost entirely arrested it, the effect lasting for a considerable period. He was thence led to try the effect of irritating mechanically and otherwise the pneumogastric nerve, and believes he has ascertained that any centripetal irritation of this nerve arrests the flow of the pancreatic secretion—that is to say, exerts an inhibitory influence on the activity of the gland.—*Lancet*, Nov. 19, 1870.

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7. *Locality of the Sense of Taste.*—Dr. CAMERER gives, in the last number of the *Zeitschrift für Biologie*, the results of his experiments on the locality of the sense of taste. He finds that the sensitiveness of different parts of the tongue depends essentially on the presence and number of the fungiform papillæ. These are most abundant near the apex of the tongue; they are less numerous at the edges of the organ, and disappear near the circumvallate papillæ. There are no papillæ on the under surface of the tongue. The mode in which Dr. Camerer conducted his experiments was to press a tube of about a third of an inch in diameter over different parts of the tongue and adjoining mucous membrane, and then to pour in a solution of the sapid substance to about the height of a quarter of an inch. Nine persons were experimented on, and the subject of the experiment did not in any instance know beforehand the nature of the solution the taste of which he was called on to determine. The substances employed were common salt, sulphate of quinia, sugar, and sulphuric acid. From his experiments it appears that the parts of the tongue that are free from papillæ possess no sensibility; also, he finds that a weak solution of a salt is more readily perceived after pure water has been tasted than after a strong solution has been tasted during the previous twenty-four hours. By touching the fungiform papillæ with a fine spiculum of salt crystal he was able to show that the gustatory sensibility resided in the fungiform papillæ themselves, and not in the adjoining mucous membrane.—*Lancet*, Oct. 8, 1870.

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8. *On the Origin and Increase of Bacteria.*—Dr. A. POLOTEBNOW has been led to the following conclusions:—

1. That a perfect genetic connection exists between *bacterium vibrio*, and *spirillum*, and that these organisms present no other differences but those of size and direction.

2. None of the vibriones (*vibrio*, *bacterium*, and *spirillum*) are independent organisms, but only derivations (delicate mycelia) from the spores of fungi, especially those of *penicillium glaucum*.

3. The development of the vibriones from the spores of *penicillium* may be best followed when the spores are exposed to the action of high temperature (140° to 212° F.).

4. The notion that vibriones are developed in the filaments of mycelium from the granules occurring in the cells proves to be quite erroneous, as also that of the conversion of vibriones into other higher forms (yeast, etc.).—*Brit. and For. Med.-Chir. Review*, Oct., 1870, from *Anzeigerd er k. k. Akad der Wiss. in Wien*, 29th April, 1869.

MATERIA MEDICA, GENERAL THERAPEUTICS, AND PHARMACY.

9. *Action of the Alkalies on the Organism.*—MM. Rabuteau and Constant presented to the French Academy of Sciences, through M. Robin, a note of some experiments made in the laboratory of the latter to ascertain the action of alkalies on the organism. During the whole period of experimenting they adhered as closely as possible to the same regimen which they had adopted some days previously.

One of them took, daily, five grammes of bicarbonate of potash, one-half of the amount at breakfast and the same at dinner, during five consecutive days. On comparing the amount of urea eliminated under the influence of this salt with that eliminated during the five preceding and the five succeeding days, it was found that the urea was at least twenty per cent. less. The number of pulsations was also diminished.

In a female who had taken, during seven days, six grammes of bicarbonate of potassa daily the urea was diminished twenty-three per cent. The pulse and temperature were also lessened. These results evidently indicate a diminution of combustion.

Finally, one of the experimenters took for six consecutive days five grammes of bicarbonate of soda daily. The diminution of urea was at times more than twenty per cent., and the beatings of the heart were rendered slower.

Further, MM. Rabuteau and Constant state that the appetite diminished so much that one of them was obliged to force himself to take the prescribed ration; and also, that marked anæmia commenced to manifest itself, especially in the female, who took in all forty-two grammes of bicarbonate of potash. This last fact, they say, proves that there was a diminution of the globules, a diminution which their previous experiments on animals had demonstrated. Finally they noticed a general debility, most marked under the action of the bicarbonate of potassa.

The experimental results they state explain a therapeutical paradox. They also explain the clinical facts differently from the hitherto admitted theory, and account for the debility caused by alkalies—

"1st. There exist a class of 'temperants,' the *refrigerentia* of Linnæus, among which are the acid fruits. But these fruits give origin to alkaline carbonates in the system; it had to be admitted that they act at first as temperants, afterwards as oxidizers. Our experiments show that these substances are temperants from the moment they enter the system until their entire elimination.

"2d. Certain essentially febrile diseases, as acute articular rheumatism, and even pneumonia, are favourably influenced by alkalies. It is known that these medicines, far from acting as stimulants in consequence of a supposed increase of oxidation, produce in these diseases a general depression—a diminution of the pulse and temperature, which coincides with our experience.

"3d. If alkalies promote oxidation they should be heroic medicines in glycosuria and in albuminuria; but alkaline waters often produce the most disastrous effects in these diseases.

"4th. The medicines which increase oxidation augment vital force. Such is common salt, which, added in excess to food, produces, according to the researches of M. Rabuteau, an increase of twenty per cent. in the urea. But alkalies produce a directly opposite effect. We may say that in many small doses they do not diminish oxidation, on the contrary appear to augment it; this we explain by their transformation into chlorides in the stomach by the action of the chlorohydric acid of the gastric juice. But then they act no longer as alkalies.

"Such are the principal results of our researches and the principal deductions which may be drawn from them. As to the explanation of the effects of the alkalies we believe it is to be found in the fact that they destroy the blood-globules, which are the carriers of oxygen, and, consequently the direct agents of oxidation."

The authors state they have nothing to add to what is already known of the effects of alkalies as lithontriptics in cases of uric acid calculi. Their action is well known. But they would protest against the opinion still held by some physicians that alkalies may be useful in all cases of calculi, even in phosphatic. "In fact," they say, "in our experiments the urine which had been generally clear under the influence of alkalies became turbid the first day after the ingestion of these medicines. This exception coincides with the statement of Wöhler, who has observed the urine under the action of alkalies deposit earthy phosphates."—*Archives Générales de Méd.*, September, 1870.

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10. *On General Anæsthesia and Anæsthetics.*—Dr. B. W. RICHARDSON communicated to the Physiological Section of the British Medical Association (August, 1870), a very valuable paper on this subject. The author arranged his remarks under the following heads: 1. Enumeration of anæsthetic substances; 2. Their mode of action; 3. The best anæsthetics, with a review of their comparative advantages and disadvantages; 4. Methods of administration; 5. Methods of promoting recovery in accidents attending administration. The substances known to produce anæsthesia when inhaled, were placed together in a table, showing, with regard to each, the chemical composition, vapour-density, fluid-density, boiling point, physical and anæsthetic qualities, and the name of the first experimenter. With regard to the mode of action of anæsthetics, the chemical composition of a vapour cannot be taken to determine its narcotic property. Bodies of very different composition—*e. g.*, nitrous oxide and benzol—act as anæsthetics. Many good anæsthetics are simple hydrocarbons; but so also are combinations of hydrogen and carbon with chlorine (chloroform), of carbon with chlorine (tetrachloride of carbon); of carbon with oxygen (carbonic oxide); etc. Snow's theory, that anæsthetics act by arresting oxidation, is not absolutely tenable; because some of the anæsthetic vapour or gases support combustion, while others arrest it. Dr. Richardson had been unable to devise any satisfactory explanation of the mode in which anæsthetics act on the sensorium. He thought it probable, however, that there were two modes; that some directly interfered with the chemical changes in the blood, while others interrupted function by immediate contact with the sensorial organs. The first class he called negative, the second active substances. Of the numerous anæsthetics which have been tried, and which were enumerated in the table, the following only were of practical use: nitrous oxide, methylic ether, methyl-ethyl-ether, ethylic ether, bichloride of methylene, chloroform, tetrachloride of carbon, methylal, bichloride of ethylene, chlorethylidine, bromide of ethyl, and amylene. The relative advantages and disadvantages of each of these were noticed. In accidents arising from the administration of anæsthetics, Dr. Richardson said that there was but one known practice that could be depended on; viz., artificial respiration without moving or disturbing the body. The double-acting bellows is the best instrument for the purpose. Galvanism both exhausts the muscular respiratory power and interrupts the action of the heart.—*British Med. Journal*, Oct. 1, 1870.

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11. *Influence of Digitalis on Nutrition.*—M. M. A. MEGEVAND has reported (*Gazette, Hebdom*, 12 Aug. 1870) a number of experiments instituted with a view of ascertaining the influence of digitalis on nutrition. He concludes from these that digitaline, and especially digitalis, notably diminishes the urea, and this diminution he considers to be closely connected with the slowing of the pulse of which it is the corollary, and, further, that it explains the antiphlogistic effects of the drug.

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12. *Physiological Properties of Momordica Elaterium.*——Dr. KÖHLER, of Halle, has just contributed a valuable paper on the historical, chemical, and physiological properties of the juice of this plant, and we present to our readers a condensed account of the last or physiological section. The activity of the juice is dependent on the presence of elaterin, which is a chemically indifferent substance analogous to the resins, and incapable of forming salts with either acids or bases. The cause of the violent purgative action of such in-

different substances as elaterin, and the analogous compounds obtained from the convolvulaceæ, euphorbiaceæ, and guttiferæ, has up to a recent period been involved in much mystery. Some clue to an explanation has, however, been obtained from the experiments of C. M. G. Zwicke on the action of convolvulin and jalappin, from which it would appear that the activity of these resins is due to their solution in the bile. Hagentorn also found that when convolvulin had been converted into a soluble convolvulinic acid it still was incapable of producing any drastic effects in cats when rubbed into the skin, or applied to the mucous membranes of the nose, mouth, or stomach, or introduced into the jugular vein. He even found that the resin might be introduced into a loop of the small intestine, the contents of which had been expressed and a ligature placed upon it, above and below, without producing vomiting or purging after twenty-four hours' stay, though the mucous membrane was reddened. Untiedt continued Hagentorn's experiments, and satisfied himself that convolvulinates of the alkalies, when subcutaneously injected or introduced into the jugular vein were inoperative. He then employed the rectum as the place of application, and found that whilst clysters of pure convolvulin, or of convolvulin and taurin, or of convolvulin and choloidinate of soda, were quite inoperative, a mixture of convolvulin and bile, or of convolvulin and glycocholate or taurocholate of soda, immediately occasioned violent diarrhœa. On the addition of soap the purging followed in four hours, and Untiedt has shown, and his statements have been corroborated by Bastgen, that in the latter case a partial conversion of convolvulin into convolvulin-oleic acid occurs. From these experiments it appears that bile, biliary acids, and soap constitute a menstruum for these resinous compounds, and their purgative action is due to the local irritation the solution in the intestinal juices excites, which solution is materially aided by the bile. Dr. Köhler gives the details of a considerable number of experiments he has himself undertaken, and demonstrates that the drastic cathartics may be divided into two, or perhaps even three, classes—namely, first into those that are, and those that are not, dependent for their activity on the presence and solvent action of the bile: the former including aloes, croton oil, rhubarb, senna, etc.; the latter being again divisible into those which, on being dissolved by the bile, act purely locally, as jalap, scammony, gamboge, agaricus, and those which, after solution in the bile, not only act locally, but are also absorbed in the blood, and act also on the nervous system; this group including the cucurbitaceæ, as elaterium, colocynth, and bryony.—*Practitioner*, Nov. 1870, from *Virchow's Archiv*, Bd, iv. heft 3, 1870.

13. *Action of Gelsemium Sempervirens.*—Dr. Roberts Bartholow, of Cincinnati, has published (*Practitioner*, Oct. 1870) some interesting experiments instituted to determine the physiological action of gelsemium on the nervous system. The following are his conclusions:—

1. Being a crystalloidal substance, gelsemiate of gelsemia, the active principle, is rapidly absorbed into the blood.
2. It has a sedative action on the nervous system.
3. It acts chiefly on the motor portion of the cord.
4. Its paralyzing effect is due to its action on the motor centre, and not to an action on the peripheral nerve-fibres.
5. It acts also on the sensory portion of the cord, producing at last complete anæsthesia; but this effect in warm-blooded animals and in man is toxic only, and follows the paralysis of the motor functions.

Synergists.—All the paralyzers are synergistic to gelsemium. In its physiological action, gelsemium corresponds more nearly to conium than to any other agent. Conium is a paralyzer, but, unlike gelsemium, the paralysis commences at the periphery, and rapidly extends to the motor centre. Conium, like gelsemium, does not destroy the muscular irritability. Gelsemium impairs the sensibility of the sensory nerves, which conium does not. Both cause death by asphyxia—paralysis of the muscles of respiration. In their effects on the brain these agents act similarly. Neither destroys, *per se*, the functions of the brain, consciousness being preserved until carbonic acid narcosis supervenes.

Both produce dilatation of the pupil and blindness; but gelsemium more distinctly paralyzes the third pair than conium.

Dr. B. has devoted much labour to determine the supposed antagonists to gelsemium. Strychnia, which, *a priori*, would seem to be a complete antagonist, is not so, as Dr. B. has convinced himself by repeated trials. An animal under the influence of gelsemium is quickly tetanized by strychnia.

Physostigma might be supposed to be an antagonist, but, on trial, the toxic effects of this article proved to be synergetic to gelsemium; death resulting more speedily from a dose of the latter when the former was also administered.

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14. *Clinical Observations on the Bromides*—Dr. NAMAIS publishes some important observations on the results of experiments made with the bromides during several past years. In 1867 he found that bromide of potassium was sometimes able to cure epilepsy, or at least that it could diminish the violence of the attacks and the frequency of their recurrence. To effect this, however, considerable doses were requisite, amounting even to as much as six drachms. In the event of any poisonous effects of the drug appearing, they vanish on the immediate stoppage of its administration in from two to three days. Bromide of potassium possesses a cumulative power, and may remain for a long time in the body, traces being discoverable for a considerable period in the urine. In 1868 the conclusions which he arrived at were—That a certain accumulation of bromide of potassium in the body of epileptic patients was required before it could produce its effects. The diminution of the accumulation of the bromide in the body can be estimated by the reduction of the quantity appearing in the urine. In one case the amount discharged in twenty-four hours by the urine amounted to a little more than fifteen grains, and he concluded from this that it was eliminated by other channels. The epileptic attacks cease when the urine contains 0.033 per cent. of bromide of potassium; they recur when the proportion falls to 0.030 per cent. The salt can also be discovered in the saliva, 100 parts containing 0.150 of the bromide. It exerts so specific an action in epilepsy as to influence it favourably even when the causes determining the attack still persist. His experiments in 1869 enabled him to add to the foregoing that the bromide not only accumulates in the fluids but in the solids of the body; that it checks reflectorial and convulsive movements, even in cases where it may be unable to exert any influence on the immediate cause of the attack; that bromide of iron corrects defective formation of blood, quiets nervous excitation, and produces the combined effects of iron and the bromides. In accordance with his present experience, Prof. Namais regards the *bromide of iron* as being in many instances a therapeutic agent of superior value in epilepsy even to the bromide of potassium.—*Practitioner*, Nov. 1870, from *Giornale Veneto di Scienze Mediche*, Ser. iii. T. 2, 1870.

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15. *Saponified Cod-liver Oil in Phthisis.*—Dr. VAN DEN CORPUT, Professor of Clinical Medicine at the St. Jean Hospital, Brussels, has just published a paper giving an account of the great advantage which he has derived from the employment of cod-liver oil saponified by lime in the treatment of phthisis. After reviewing the published accounts of the beneficial action of cod-liver oil, and of the various means which have been adopted (with only a certain amount of success) for overcoming the repugnance which so many patients feel for it, he refers to some experiments which are but little known, although of a high therapeutic value, which were made in 1857, at the Artisan's Hospital, of Moscow, of treating phthisis by calcined bone. Of ninety women with tubercular cavities, twenty-five quitted the hospital cured, or at least in a condition of health relatively good. He attributes also to the presence of the same substance in James's powder "its recognized efficacy in certain cases of phthisis, the reputation of which is justly maintained in our own day," an admission which, we think, few will indorse here. Certain calcareous mineral waters are also debited with the same efficacy. As a first result of his own investigations in this direction, Professor Van den Corput found that the following formula furnished a preparation that is easier taken and is more efficacious than the oil taken alone, and he has made use of it in his hospital: ℞.—Cod-liver oil 250,

chloride of calcium 4, essence of bitter almonds or anise 2 parts. Two or three tablespoonfuls to be taken daily after meals, shaking the bottle well each time.

While employing this preparation, however, he had occasion to observe the excellent results obtained by Professor Namais, at the Venice Civil Hospital, by the simultaneous employment of lime-water and cod-liver oil, and the idea occurred to him of uniting these two elements of treatment into a solid combination, facilitating their ingestion and assimilation. He enters at some length into the consideration of the probable effects of this compound upon the transformation of tissues; but for this we have not space, and prefer giving the author's formula for his *jecoro-calcaire savon*. By this, 100 grammes of pure cod-liver oil are ordered to be saponified into a pilular consistence by means of hydrated lime, and then aromatized by 1 gramme of bitter almond or anise essence. The mass is to be divided into boluses of 25 or 30 centigrammes, which may be rolled in a mixture of sugar and iris powder, and covered, if required, by ethereal tincture of tolu. From six to ten of these are to be taken daily, two at a time, immediately after a meal. In certain cases, morphia, aconite, henbane, or any other substance that is indicated may be associated with the *savon*.

"It is only," the Professor adds, "after having been able to verify, during several years, and upon some hundreds of patients, the favourable action of this medicament, compared with the treatment by the simple oil and other therapeutical agents, and after having received the favourable testimony as to its benefit from those *confrères* to whom I had communicated my ideas, that I have resolved to give publicity to this new mode of treating phthisis. Without desiring in any manner to regard it as a specific, and still less as infallible, I am in a good position to affirm that this method, aided by suitable regimen and hygienic conditions, is founded both on rational theoretic reasoning and on a sufficient number of facts to allow of positive conclusions being drawn. . . . . It is especially at the commencement of tuberculization that the *savon* proves of real utility. Administered on the first appearance of the symptoms of the disease, and continued for a more or less prolonged period, according to the subjects, and especially the nature or degree of the lesions, I have almost always found it retard the progress of the affection, amend in a notable manner the organic degradation and, consequent upon the modification of the nutritive process, often (*parfois*) lead to a cure—or, at least, to a more or less prolonged arrest of the pathologic evolution of tuberculosis. At a more advanced period its action, though less efficacious, has seemed to appease in a remarkable manner some of the symptoms. One of the most evident effects is to most sensibly diminish the cough and expectoration, to moderate the hectic fever, and to restore the strength to its normal diapason by favouring interstitial nutrition in a remarkable manner.

"The direct advantages of the preparation as a medicinal agent are its solid form and its slight and not disagreeable taste. In consequence of its easier ingestion and assimilation, it does not disturb digestion, and does not give rise to the diarrhœa so often caused by cod-liver oil. Still, when after its prolonged employment it begins to excite a certain amount of repulsion, it should be suspended for some days, and in such cases pancreatine or bitters may be associated with it with advantage. The preparation is contraindicated when in very advanced phthisis there is excessive hectic, and the digestive organs have already undergone serious alterations. It should also be suspended when the hæmoptysis exists. Still, even in the third stage of the disease, the *savon jecoro-calcaire* produces in many cases a very marked sedation of most of the hectic symptoms. Usually, diarrhœa ceases or diminishes, the evening exacerbation of temperature declines, the colliquative sweats diminish, and the weight of the body sensibly increases. Sometimes, even, the remedy has seemed, after prolonged employment, to have led to the cicatrization or drying-up of cavities of moderate size. It is especially in torpid phthisis, in that form based on lymphatism, which constitutes its most common appearance, especially in our climate, and which is usually accompanied by hyphæmia, that I have found the *savon* of true utility. It has seemed to me to exert no real efficacy in acute phthisis, which happily is a much rarer affection. The duration of treatment—which may,

indeed, be indefinitely prolonged—varies, as a general rule, from six weeks to three months. Generally, after two or three weeks, the *gargouillements* diminish, the cough is less frequent, the expectoration is modified, and, after a time, the fever ceases or at least diminishes. The appetite is almost always preserved, or even increased.—*Med. Times and Gazette*, Nov. 26, 1870.

16. *Styptic Wool.*—Dr. EHRLE, of Isny, prepares this by boiling the finest carded wool for half an hour or an hour in a solution containing four per cent. of soda, then thoroughly washes it out in cold spring water, wrings and dries it. The wool is thus effectually purified, and is now capable of imbibing fluids uniformly. It is then to be dipped two or three times in fluid chloride of iron diluted with one-third of water, expressed and dried in a draught of air, but not in the sun or with the aid of high heat; finally it is carded out. Thus prepared, it is of a beautiful yellowish-brown colour, and feels like ordinary dry cotton wool. As it is highly hygroscopic, it must be kept dry, and when required to be transported must be packed in caoutchouc or bladder. Charpie may be prepared in a similar manner, but, on account of its coarser texture, is not so effective as cotton wool, presenting a less surface for producing coagulation. When the wool is placed on a bleeding wound, it induces moderate contraction of the tissue, coagulation of the blood that has escaped, and subsequently coagulation of the blood that is contained within the injured vessels, and thus arrests the hemorrhage. The coagulating power of the chloride of iron is clearly exalted by the extension of its surface that is in this way effected. The application of the prepared wool is not particularly painful, whilst, by sucking up the superfluous discharge and preventing its decomposition, it seems to operate favourably on the progress of the wound. The unpleasant secondary results that have led many practical surgeons to discard the use of the perchloride of iron do not occur with the wool when it is properly made and applied. In cases of wounds where the bleeding proceeds from large and deep-seated vessels, it may be used as a compress, a bandage being applied over it, or the wound may be plugged with it. It may also be employed with advantage in cases of profuse suppuration, to imbibe the discharge and purify the surface. He recommends that a small portion should be given to every soldier on going into action.—*Lancet*, Nov. 12, 1870.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

17. *On Local Inflammation in Certain Defined Conditions as Causes of Pulmonary Phthisis.*—Dr. ANDREW CLARK read a paper on this subject before the Medical Society of London (Oct. 17). He stated that hitherto his illustrations had been drawn from cases of pneumonia and pleurisy, and as yet he had found no exception to the laws he had set forth as regulating these relations to phthisis. In the present paper he proposed to take his examples from chronic bronchitis, which, though a less common was an equally efficient agent in bringing about phthisical destruction of the lung. Case 1, a widow, 48 years of age, admitted into the London Hospital for chronic bronchitis, in June, 1866. She has had winter cough for twelve years back. Percussion sounds good over both lungs; with the stethoscope sibilant and sonorous râles are heard everywhere, and at bases there is coarse, moist crepitation. Pulse 80; no fever or night sweats. She was treated with alkalies, and afterwards with acids, iron, and creasote inhalations, and was doing well until she was placed on a more liberal diet, with beer and wine. It being obvious that the change did harm, the diet was reduced; this displeased her, and she left the hospital. In three months she was again admitted under another physician; she got worse, became feverish, and in February, 1867, dulness on percussion in the supra-spinous fossæ was noted. In March lung tissue was found in the expectoration, she

got diarrhœa, and died in the May following. The post-mortem examination revealed extensive disease in the left lung; the bronchi were thickened and dilated, and the intervening lung tissue was converted into a dense fibrous mass, a few small cavities were seen, and also some gray tubercles in both apices. Case 2.—J. S., aged 65, a widow, had winter cough for ten years. On admission, had much cough, and muco-purulent expectoration; no dulness on percussion; sibilant râles over both lungs; pulse 64; temperature 98°. This patient gradually declined, owing to a nervous shock; she got hæmoptysis and diarrhœa, and signs of consolidation about right lung; she died, but no post-mortem was obtained. Case 3.—J. W., aged 53, a dock labourer, had bronchitis over twenty years. Four years ago his strengh failed; he got profuse purulent expectoration. He became feverish; bronchial breathings and crepitations were heard over his chest, and right lung became consolidated about two years later. The sputa became lumpy, and contained areolæ of elastic tissue. In December, 1869, the urine was found to be albuminous, and in the following April he died. The post-mortem showed dilated tubes, gray tubercles, and small cavities in the lungs. Case 4.—Mrs. B. aged 61, lives in Kent; for many years subject to bronchitis. In 1864, general health failed, she became feverish, and bands of elastic tissue were found in the sputa. Crepitation heard in middle third of right lung. The progress of the disease here is slow; pulse 80; temperature 98.2° at 3 P. M. The patient is still under observation. In concluding, Dr. Clark said that the chief causes of phthisical complication, in cases of chronic bronchitis, appear to be repeated colds, over-feeding, and the abuse of stimulants. Loss of strength, feverishness, afterwards subsiding and coarse moist crepitations, were signs of the phthisical condition. When the bronchi ulcerate, fibres of lung tissue are found in the sputa; fibroid or tubercular pneumonic changes in the lung follow, but the progress of the disease is usually slow. By meeting feverish complications with rest, milk diet, and salines, and by the use of inhalations of iodine, creasote, or carbolic acid, and appropriate treatment of tonics and diet, the progress of the disease may be greatly retarded, and life indefinitely prolonged.—*Medical Times and Gazette*, Nov. 5, 1870.

18. *Treatment of Catarrhal Pneumonia of Infants at the Breast by Carbonate of Ammonia.*—Dr. Stierlin observes that the danger of catarrhal pneumonia (bronchopneumony, lobular pneumonia) in infants, whether consequent on measles, hooping-cough, or bronchial catarrh, is due to the circumstance that the bronchiæ become plugged, carbonic acid narcosis succeeds, and too small a quantity of blood enters the left ventricle, which exerts a paralyzing influence upon the heart. Dr. Stierlin formerly treated such little patients with emetics, as ipecacuanha and vinum antimoniale, calomel, and mustard poultices, but with very indifferent results. But after he had begun to administer ammonium carbonicum (carbonate of ammonia), and ammonium carbonicum pyro-oleosum, he was so successful in the treatment of this disease, which according to Rilliet and Barthez, is absolutely fatal during the first year of life, that of 150 patients only 7 died. He usually prescribed the ammonium carbonicum to the extent of 20 grains; or the liquor ammonium carbon. pyro-oleosi to the extent of half a drachm in two ounces of fluid, of which a teaspoonful should be taken every hour. During the first year of life, besides the above, he applied sinapisms to the chest. In older and stronger children he also prescribed emetics. He never observed any disadvantageous effects.—*Practitioner*, Nov. 1870, from *Berliner Wöchenschrift*, 1870, No. 26.

19. *Acute Pulmonary Tuberculosis.*—Dr. A. Steffen pronounces (*Jahrbuch für Kinderheilkunde*, N. F. III., 1870), in opposition to the conclusions arrived at by other observers, the disposition to tuberculosis of the lungs to occur especially during the earlier stages of life, namely, between the second and third year after birth; the disposition diminishing towards the period of puberty. He remarks, also, that the acute form of pulmonary tuberculosis seldom occurs independent of tubercular depositions in many of the other organs, especially in the spleen and pia mater; when meningeal tuberculosis is present, unattended with tuberculosis of the lungs, in general, in all other portions of the body tuber-

cles are absent. In pulmonary, meningeal, and choroideal tuberculosis a correct diagnosis may be arrived at. The absence of choroideal tuberculosis is no proof of the non-existence of meningeal tuberculosis, which may be present in the absence of the former. The occurrence of the latter is, in general, indicated by an entire change in the previous symptoms. Tuberculosis of the lungs, though in early life it seldom occurs alone, is not, however, to be set down as the original source of the tubercular deposit in the pia mater and other locations. It is somewhat difficult, occasionally, to detect the presence of tubercles in the choroideal coat of the eye, in consequence of their excentric position. In order to bring them into view in such cases it is necessary to dilate the pupil by atropia. The restlessness of the child often increases greatly the difficulty of making a satisfactory examination of the eye, and of repeating it at proper intervals to enable us to watch the increase and progress of the tubercular deposit. In the diagnosis of acute tuberculosis of the lungs in early life, physical exploration is of little value; the presence of tubercles in the lungs is to be inferred, chiefly from the constantly increasing emaciation, the extreme slowness and constantly variable condition of the pulse and respiration, by the proportionately slight increase in the temperature of the surface, and by the absence of the pathognomonic symptoms of other forms of disease. In the course of acute pulmonary tuberculosis the intense dyspnœa, with the violent efforts to overcome it, is liable to give rise to more or less extensive emphysema of the lungs. It seldom amounts, however, to the interlobular form, and still less to the subpleural. A heretofore undescribed form is referred to by Dr. S., in which there took place a perforation of the pleura, allowing the contents of the subpleural air-vesicles to escape into the cavity of the chest, and thus giving rise to pneumothorax. It occurred in a girl three years old.—*Centralblatt f. d. Med. Wissenschaften*, July 16, 1870.

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20. *The Local Application of Carbolic Acid in the Treatment of Phthisis Pulmonalis and Diphtheria.*—Dr. C. G. ROTHE, in the *Berliner klinische Wochenschrift*, 1870, 23 u. 24, states that, prompted by the favourable results obtained from the employment of Lister's bandages in the treatment of ulcerated surfaces, he was prompted to make trial of inhalations of carbolic acid in cases of tuberculosis of the lungs, and speaks highly of their beneficial effects. Out of the six cases of phthisis reported, three were cured, notwithstanding the already advanced stage in which the disease presented itself in all of them. The others were probably cases of caseous pneumonia (*käsige pneumonien*) occurring during and hastening the fatal course of the tuberculosis. This must be received only as a probable conjecture, inasmuch as no post-mortem examination was made. The formula in which the carbolic acid was used by Dr. R. is as follows: Acid. carbolic. cryst., spir. vini, āā 1.0–2.0; aq. destill. 5.0; tinct. iodi 1.0.—M. Of this 10, 15, or 20 minims, with 30.0 of water, is to be inhaled. The addition of the iodine is merely for the purpose of abating the disagreeable smell of the carbolic acid. Dr. R. presents the following as a general summary of his observations as to the efficacy of inhalations of carbolic acid in pulmonary phthisis: 1. The earlier in the disease the less will be the extent of the ulcerated surface, and the more nearly will it approach in character to that of a local lesion, and the greater the prospect of a successful result. 2. The inhalations may be repeated from four to six times daily, for months. They should never be employed to the exclusion of such remedies as may be called for by any of the occurring symptoms. 3. The presence of fever and dyspnœa does not forbid the use of the inhalations; but when, from sudden exposure to cold, there ensues a catarrhal irritation of the entire extent of bronchial mucous membrane, it would seem that but little benefit is to be expected from the inhalations. 4. Hæmoptysis will with the greatest promptness be arrested by the inhalation of the chloride of iron; still, it is not improbable but that the inhalation of carbolic acid will act in such cases as a styptic; we know that creasote has been long employed as such in epistaxis. 5. The pertinacity with which the smell from the fumes of carbolic acid continues to pervade the atmosphere of closed apartments renders the breathing of such atmosphere beneficial in cases of diseased lungs; it is possible that, in persons predisposed to pulmonary consumption, occupying

an apartment filled with a similar atmosphere, the disease may be prevented. 6. The occurrence of bad symptoms is not to become a cause of despair.

In the same formula as above, Dr. R. directs the use of the carbolic acid in cases of diphtheria, but not by inhalation. It is to be directly applied to the seat of the disease by means of a hair pencil. At the same time the patient is to use as a gargle 10 to 15 drops of the above mixture to a tumbler (*grosse tasse*) of water. Previously to the introduction of this treatment nearly all Dr. R.'s cases of diphtheria terminated fatally; since its introduction, they have all run a favourable course.—*Centralblatt f. d. Med. Wissenschaften*, Sept. 17, 1870.

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21. *Tubercular Deposit in the Cerebellum.*—Dr. Simpson read to the Manchester Medical Society, notes of a case of cerebral disease of much interest. The patient was a boy aged 14, who, up to the end of 1869, had enjoyed good health, and whose family history was satisfactory. About Christmas he began to complain of severe shooting pain at the back of the head, intermittent in character, and at times so severe as to make him scream out. Soon afterwards he began to vomit; at first in the morning, but subsequently also after his meals. The vomiting was unaccompanied by nausea. His bowels became very costive. He continued in this state for some time, when, about the middle of April, he began to have a dull heavy expression, and also showed some difficulty in walking and articulating. It soon became evident that his sight was becoming impaired, and though he remained fairly intelligent, his memory became very defective. There was slight paralysis of the internal rectus of the left eye. The symptoms gradually became worse. His gait was not that of paralysis, but of want of co-ordinating power. On July 25th, his eyes were examined with the ophthalmoscope, by Mr. Windsor, who reported a large and swollen condition of the optic disk, with no distinct boundary, a swollen and tortuous condition of the veins, and other evidence of intracranial pressure. His progress was steadily downward, and he died on September 17th, comatose. At the *post-mortem* examination the head only could be examined. The veins of the dura mater were turgid, and the visceral arachnoid rather thick and opaque; and there were several ounces of slightly turbid cerebro-spinal fluid. There was a little lymph about the optic commissure and anterior margin of the pons. The venæ Galeni were very turgid. The whole brain substance was softened, particularly the parts at the base. Both lobes of the cerebellum contained numerous yellow masses of tubercle, varying in size from a pea to a marble. No tubercle was found elsewhere in the brain, and during life there was no evidence of its presence in any other organ. Dr. Simpson referred to the question of diagnosis, and discussed particularly the differential diagnosis of tubercle and hydatid cyst of the brain.—*British Med. Journal*, Oct. 22, 1870.

22. *Tubercle of the Spinal Cord.*—Dr. Eisenschitz, in the *Jahrbuch f. Kinderheilk.*, iii. 2, 1870, reports the case of a male child, 3½ years old, who after suffering for some three days from severe headache and wakefulness, was seized with partial paralysis of the lower extremities, and of the urinary bladder. There was diminished sensibility of the paralyzed limbs; with complete insensibility of all the parts below the eighth dorsal vertebra, rendering the child, otherwise endowed with great irritability of constitution, perfectly passive under the most painful manipulations of the parts thus affected with analgesia. The activity of the reflex nervous functions were altogether unimpaired. Decubitus was in consequence maintained. At the end of six weeks death ensued. The examination of the body after death revealed the existence of a condition of general tuberculization. Throughout nearly the entire extent of the cerebral pia mater there was a diffusion of miliary tubercles. Two cheese-like tumors, the size, nearly, of beans, were found imbedded in the right hemisphere of the cerebellum, and a similar tumour of the size of a pea in the spinal marrow, towards the lower part of its dorsal portion. That at this part of the cord there existed a lesion of, at least, the gray portion of the cord, was very evident during the lifetime of the patient. The white substance of the cord

was merely enlarged and slightly softened. There was no indication of disease in either of the meninges of the spinal cord.—*Centralblatt f. d. Medicinisch. Wissenschaften*, July 23, 1870. D. F. C.

23. *Tubercles in the Peritoneum.*—Dr. ALBANUS presents (*St. Petersburgh Med. Ztschr.*, 1869) a sketch of the pathology of this disease based on the careful study of twelve selected cases. He objects to the name "*peritonitis tuberculosa*," usually applied to the affection, inasmuch as the production of the tubercles is entirely independent of any inflammatory action. In what is usually termed the first stage of tuberculization of the peritoneum, the latter exhibits simply an increase of *productiveness*, by which there is developed upon its surface numerous grayish-coloured miliary eminences, while, at the same time, there takes place an abnormally augmented secretion of serum into the peritoneal sac, so that from an early period some degree of ascites is already present. In the second stage the tubercular deposits acting as foreign bodies, set up an irritation in the peritoneum, and become surrounded by an augmented vascularity in the form of a red halo, indicative of the commencement of inflammation, the ascites, in the meantime, increasing in extent. In the third stage there occur softening and disintegration of the tubercles, an increase in the peritoneal inflammation, and a change in the effused fluid, it becoming turbid and of a reddish colour. In a clinical study of the disease this division of its course into different stages is not, however, recognizable.

The attack of tuberculization in the peritoneum commences with a general feeling of malaise, which, however, presents nothing to indicate with any precision the onset of so important a disease. Only in one case was an initial chill observed. When there is already present tubercles of the lungs or any other organ, still more obscure are the indications of abdominal disease. More or less speedily, however, symptoms are developed which indicate the locality, at least, at which morbid action has commenced. The entire establishment of the disease gives rise to a distressing, dragging sensation in the abdomen; soon after is added pain in the abdomen and loins, with loss of appetite, diarrhœa, and evident intumescence of the abdomen, the cause of which is soon recognized to be the accumulation of serum within the peritoneal sac. The patient becomes now feverish, the diarrhœa increases; there take place, occasionally, nausea and vomiting; the appetite continues defective. As the ascites increases in extent, the pain of the abdomen augments in severity, and bronchial catarrh with dyspnœa sets in. The fever increases with the full establishment of peritoneal inflammation. The lower extremities become œdematous. As the above symptoms augment in intensity, rapid emaciation ensues with great exhaustion, night sweats, etc. Remissions often occur in the course of the disease; the ascites begins to diminish; the discharges from the bowels are less frequent; the fever becomes diminished in intensity, and convalescence appears imminent. This apparent amendment, however, is of short continuance. The effusion into the cavity of the abdomen, the diarrhœa, the local pain, and the fever are the essential symptoms upon which, in cases of tubercles of the peritoneum, a correct diagnosis is to be based. Dr. A., remarks his reporter, Dr. Fräntzel, omits to mention among the diagnostic symptoms of tuberculization of the peritoneum, the frequent occurrence of a thickening, extending like a ridge across the abdomen, which can be felt by the hand through the abdominal parietes, as though it were a rope passed around the viscera.

In respect to treatment, the entire phenomena of the disease indicate the employment of roborants and a nourishing diet. Paracentesis, Dr. A. found to be injurious rather than beneficial. He proscribes it, therefore, entirely, excepting in cases where the effusion within the abdomen is to such an extent as to crowd upwards the diaphragm, thus causing an amount of dyspnœa threatening, almost, suffocation.

We may remark that all of the twelve cases upon the close observation of which the account given by Dr. A. of the pathology of peritoneal tuberculization is founded, terminated fatally.—*Centralblatt f. d. Medicinis. Wissenschaft.*, July 30, 1870. D. F. C.

24. *Pathology of Angina Pectoris.*—MM. A. Eulenburg and P. Guttman discuss (*Archiv für Psychiatrie*, t. ii. 1869) all the facts known and the theories advanced in regard to this affection, and they conclude that it is a neurosis both of motion and sensation. The symptoms to which it gives rise may be excited by causes of a different nature, even extraneous to the heart. All the cardiac nerves are probably more or less affected in this disease, and the variation in the phenomena observed in different individuals results doubtless from the more or less active part taken by the nerves which unite together in the cardiac plexus in the production of these phenomena. It is probable that the *great sympathetic* performs the most important part, inasmuch as it constitutes the larger portion of the cardiac plexus.—*Archives Générales*, Sept. 1870.

25. *Intermittent Fever latent for several Months.*—Prof. W. Braune states that of twelve persons who, in the autmn of 1868, had resorted to Borkum, an East Friedland island, for the purpose of taking sea-baths, eleven were attacked with intermittent fever. But while two of the eleven had the fever whilst on the island, the other nine did not suffer from it till the following spring and summer—*i. e.*, from six to nine months after their stay at Borkum. The patients were adults, all of high social position, and most of them females, and belonged to various localities, as Leipzig, Halle, Liegnitz, Atteburg, &c.; and in these places, when the patients severally residing in them were attacked, intermittent fever did not prevail, or was present in only a few sporadic cases. None of the patients had ever before had intermittent fever. It seemed, therefore, that in all of them the malady must have had its cause in malarious poison received during their autumnal stay in Borkum. The summer had been there, as elsewhere, very hot, and there was a considerable scarcity of pure water fit for drinking. The professor remarks that a prolonged period of incubation or latency after malarious poisoning is not so rare as some authorities, as Griesinger, have supposed; and in support of this he appeals to the observations made by Pfeiffer in the army of Weimar, published in a *Jeniaschen Zeitschrift*, 1868. Twice—in 1849 and 1867—certain divisions of the troops suffered an epidemic of intermittent fever, which must have been due in each instance to exposure to malarious poison in the autumn of the preceding year.—*Brit. and For. Med.-Chir. Review*, Oct. 1870, from *Archiv f. Heilkunde*, Dec. 1869.

26. *Vaccinia and Varioloid at Eleven Days' Interval.*—M. Broardel communicated to the French Academy of Sciences (22d July, 1870) the following interesting example of such occurence: A woman was admitted into l'Hôpital la Pitié, July 5, 1870, and the next morning at four o'clock was delivered of twins. About noon of the same day both were vaccinated with Jennerian virus from a healthy child, one with nine, the other eight punctures, all of which took; and eight days afterwards (July 13) virus was obtained from them for vaccinating ten or twelve infants. On the 19th of July the twins simultaneously exhibited red spots on their skin which proved to be the commencement of a varioloid eruption. Both children recovered. Both had well-marked vaccine scabs on their arms while suffering from varioloid.—*Archiv. Gén.*, August 30, 1870.

27. *Hemiplegia from Cerebral Hemorrhage in Cases where there is Valvular Disease of the Heart.*—The mere fact that hemiplegia has occurred in a person, even a young person, the subject of valvular disease of the heart, must not alone lead one to the diagnosis of embolism. The mode of onset is the important matter in the diagnosis of the nature of the lesion in all cases of hemiplegia. If we learn that the patient had been deeply insensible for many hours, or, more generally, if he had been for some time in the "apoplectic condition," the diagnosis of cerebral hemorrhage is at least as likely as that of plugging, notwithstanding that there is valvular disease. The bleeding is from an aneurism of the middle cerebral artery, or from a branch of it. It is true that, as a rule, such aneurisms burst outside the brain, and do not produce hemiplegia, but now and then they imitate, so to speak, ordinary cerebral hemorrhage, and break up the motor track. Dr. Hughlings Jackson's observations confirm the opinion of Dr. John W. Ogle and Dr. Church as to the association of aneu-

risms of the larger cerebral arteries with vegetations on the heart's valves. Dr. Hughlings Jackson thinks that the kind of defect of speech which sometimes accompanies hemiplegia is useful evidence as to the *pathological* nature of the lesion. *Loss* of speech—loss excepting the patient's stock word or phrase—is in favour of hemorrhage. Still, plugging will produce complete and permanent loss of speech. If the patient recover, and if during recovery he make frequent mistakes in words—the so-called loss of memory for words—articulating them clearly, Dr. Hughlings Jackson thinks the diagnosis of plugging more warrantable than that of clot.—*British Medical Journal*, Oct. 29, 1870.

28. *Differences in Nervous Symptoms from Differences in the Pathological Nature of Lesions.*—It may seem at first glance that the *pathological* nature of the lesion is of no consequence so far as the kind of symptom goes. Of course, if the very same part of three nervous systems were suddenly destroyed by three different lesions, the resulting symptoms would be the same in the three cases. But practically this does not happen. Although clot, softening from embolism, and tumour, may each damage the region of the corpus striatum, they do not affect the very same parts of this region, and they do not act on it in the very same way. For instance, clot destroys locally, it destroys suddenly, and it squeezes quickly. It mostly destroys more or less of the corpus striatum, and mostly leaves convolutions undamaged, although split off from the motor centre. Softening from plugging occupies arterial districts, and different districts, according to the branch of the Sylvian artery plugged. It may affect convolutions as well as the motor centre. Moreover, it will *alter* the nutrition of some parts while it arrests that of others. After partial or seemingly complete recovery from hemiplegia due to embolism, the patient becomes occasionally liable to chronic convulsive seizures, beginning in the face, or arm, or leg of the side previously paralyzed. This liability does not occur from clot. A tumour may grow anywhere in the district; and, if it grow to a large size very slowly, even in the convolutions near to the corpus striatum, it need produce no symptoms. Nervous organs bear slow squeezing very well indeed; they resent sudden squeezing. The slow growth of tumours may, Dr. HUGHLINGS JACKSON thinks, account for the absence of speech-defects in some cases of disease (especially syphilitic disease) near to the left corpus striatum.

The *altered* nutrition above spoken of is probably owing to blocking of small arteries, which leads to congestion and even to extravasation *beyond* the block. Hence, in cases of embolism of a large branch of the middle cerebral artery, we occasionally find not only softening and destruction of fibres, but also evidences of rupture of small vessels—"red softening" in the convolutions. Sometimes a considerable part of the pia mater is stained of an ochre colour from old extravasation. The expression "altered nutrition" is used because, although "increased nutrition" will result from a locally increased quantity of blood, such congestion will in all probability lead to new matter of Composition, differing from, although of the same Constitution as (using the words in their chemical sense), that of health.—*Ibid.*

29. *Acute Atrophy of the Liver and Phosphorus Poisoning.*—Dr. BOLLINGER describes two cases of phosphorus poisoning and two others of acute yellow atrophy of the liver, according to the results exhibited in their investigation, both macro and microscopically, and gives, thereupon, a summary of these diseases, and especially of the opinions hitherto brought forward of the origin of the so generally concomitant icterus. Contrary to numerous assertions the author notes the absence of the overgrowth of small cells of the interstitial tissue of the liver. In regard to the icterus the author declares for himself that it is in both diseases a *resorption-icterus*, and certainly not, as Virchow has in most cases made it to be received, as being set up by swelling of the mucous membrane at the outlet of the ductus choledochus, but by parenchymatous inflammatory fatty degeneration of the liver cells, and throwing off of the fatty degenerated epithelium of the fine biliary canaliculi. The theory of the hæmatogenous icterus appears very doubtful, as in the liver the effect was

always sufficient to demonstrate the origin of the icterus.—*Brit. and For. Med.-Chir. Review*, October, 1870, from *Centralblatt für die Med. Wiss.*, April, 1869.

30. *Injury of the Liver resulting in Abscess.*—Dr. John Harley read a paper on this before the Clinical Society. The patient, æt. 69, was, by the fall of his horse, thrown forwards upon the brass turrets of his horse's pad, thus bruising the epigastric region. An inflammatory tumour, exquisitely painful, formed just above and to the *right* of the umbilicus. On moving in bed fourteen days after the accident, this tumour suddenly disappeared; collapse followed, and subsequently general peritonitis. In a few day the peritonitis became circumscribed, and the pain and swelling limited to the *left* side of the umbilical region, where there was evidence of formation of a second abscess. This also suddenly disappeared on the twenty-fifth day, with dangerous collapse and evacuation of pus by the rectum. Marked colic symptoms, showing lesion of the bowel, preceded this second rupture. From time to time there were retention of the purulent discharge and hectic, but ultimately a free passage was maintained, the appetite returned, and the patient made so complete a recovery that, at the end of seven months from the time of the accident, he was able to ride and follow his professional work, and he has since (for the last eighteen months) enjoyed perfect health. There was interrupted discharge of pus by the rectum up to the 112th day. The author considered that the left lobe of the liver was bruised or lacerated by the accident; that an abscess formed which pointed partly towards the right hypochondrium, and partly within the small omental sac behind the stomach; that, rupture having occurred in the latter situation, the pus escaped into the small omental sac, and then, finding its way though the upper layer of the transverse meso-colon, passed further downwards between its layers, forming a second abscess by the side of the descending colon. This, in its turn, caused inflammation and rupture of the contiguous wall of the large intestine, and thus the pus was liberated and carried out of the body. Hectic and jaundice attended the retention of the discharge.—*British Med. Journal*, Oct. 22, 1870.

31. *Parenchymatous Nephritis in Early Childhood.*—The *Journal f. Kinderkrankh.*, 1870, copies from the *Nordiskt. Med. Arch.*, a paper by Dr. Kjellberg, of Stockholm, in which he remarks that the opinion advanced by some writers, that in early childhood nephritis is of unfrequent occurrence, is erroneous. In 696 cases of death in young children, observed between the years 1863 and 1868, parenchymatous nephritis was found to be present, on post-mortem examination, in 126, or 18.1 per cent. In no one of these cases was the disease of the kidneys consequent upon either scarlatina or measles. In the greater number of cases, 46.85 per cent., it occurred in children who had laboured under acute and chronic intestinal catarrh. The next greatest number, 29 per cent., in those who had suffered from syphilis; then 18.18 per cent. in such as suffered from pyæmia; then 16.65 per cent. in such as had laboured under meningitis; then 14.63 per cent. in erysipelatous patients; then 12.69 per cent. in such as had been affected with pneumonia and pleuritis; then 11.76 per cent. in such as had fallen victims to capillary bronchitis. From an attentive clinical study of thirty cases of nephritis, complicating chronic gastric catarrh, Dr. K. presents a picture of its pathognomonic traits, as follows: The patient lies in a state of more or less complete sopor or coma, with half-closed eyes, the visible portion of the cornea being smeared in many cases with mucus. The orbits somewhat œdematous; the ball of the eye, as it were, deeply sunk; the pupils respond slowly to light; the folds of the alæ nasi project; the nose cool and pointed; the cranial fontanelles depressed; the neck is somewhat bent backwards, with the occiput ground into the pillow; the veins of the head are more prominent than usual. At times the child mutters to itself or cries out with a hoarse noise, or he utters, apparently without motive, sharp screams, and falls often into convulsive paroxysms; the mouth is half open; the tongue dry; to these is added sometimes vomiting. The abdomen often swollen and tense, but most generally flat, soft, and the bowels indolent; respiration is slow

and irregular; the skin cool, dry, but without elasticity—a fold of it being raised, this only slowly disappears. The extremities have a cool feel; the lower ones being often extended and stiff; frequently œdematous. The secretion of urine is diminished; sometimes nearly suspended. The urine is charged with albumen to a greater or less extent, and in its sediment there is detected by the microscope, dark-coloured granular bodies, as well as hyaline cylinders, and usually a great number of small round cells strewn throughout or collected in masses.

That a part of these symptoms are not those which arise from disease of the bowels is certain. When, therefore, in any case the foregoing group of symptoms is present, the question occurs, to what is to be referred the symptoms which cannot be accounted for by the presence of stomachic or intestinal disease? whether do they originate from a complication of the last-named disease with disease of the kidneys or the brain? In answer to the question as to their cerebral origin, Dr. K. refers to the very dissimilar and often negative condition of the encephalon found after death. Dr. K. believes that the so-called brain symptoms, such as are met with in similar cases to those referred to, are of uremic origin. In cases of intestinal catarrh uncomplicated with nephritis they are not present. The group of symptoms described by Dr. K. favour, he remarks, those present in the case described by Dr. Marshall Hall, under the name of hydrocephaloid disease, the cause of which he supposes to be anæmia of the brain. Whether the latter affection is identical with that described by Dr. K., he is unable to decide, as Dr. Hall made no reference to the condition of the kidneys, his description is based upon the observation of only a single case.

In respect to treatment, Dr. K. recommends immersion in hot baths of from 38° to 42° C., with the addition of powdered mustard, followed by the application to the region of the kidneys of a meal poultice. When there is deficient action of the kidneys, he substitutes for the poultice stupes wet with tincture of digitalis, and renewed every three or four hours. Internally, he directs stimulants and tonics, always, however, having strict regard to the nature, extent, and condition of the bowel affection.—*Centralblatt f. d. Med. Wissenschaften*, July 16, 1870.

D. F. C.

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32. *Addison's Disease and Sclerodermia.*—Dr. J. M. ROSSBACH, of Würzburg, relates (*Virchow's Archives*, August, 1870) a case of the coexistence of these two diseases in the same patient, but during life the symptoms of Addison's disease were the more prominent. There was no disease of the suprarenal capsules discovered after death. The state of the skin termed sclerodermia existed in this case mostly on the extremities, and consisted in a conversion of the subcutaneous adipose tissue into firm connective tissue, with hypertrophy of the smooth muscular elements of the skin. The pigmentation of the skin presented some marked peculiarities. In the greater number of recorded cases of sclerodermia there has been no pigmentation, and where it did exist the colour was more of a brown, or yellowish-brown, than the deep bronze of Addison's disease. In the author's case, the colour, where it existed, was very intense, but it was almost entirely absent in the parts where the sclerodermia was present, so that in respect to pigmentation the skin exhibited the peculiarities of both diseases. Again, the usual seat of increase of pigment is the rete mucosum, forming a simple increase or hyperplasia of the preëxisting pigment; in some cases, however, it has been observed to a less extent in the cutis itself; but in the case related the pigment not only existed in the cutis, but was greater in amount there than in the rete mucosum, being especially developed around the sweat-glands. The subject will be concluded in a succeeding paper.—*Glasgow Med. Journal*, November, 1870.

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33. *Partial Atrophy of the Face.*—Dr. LOUIS LANDE has collected records of several cases of a rare affection which was first described by Parry in 1825, and has since been noticed by Romberg, Stilling, and others. The following case was recorded by Guttman: "A. R., æt. 18, of a healthy family; up to the age of 11 in perfect health. At this time the left cheek assumed a pale tint, and began to diminish slowly. At the end of three years medical advice

was obtained, and stimulating applications and electricity were tried, but without effect. The disease made progress for five years and then stopped. At the end of that time he presented the following condition: The muscular system is well developed, and, except in the face, the body presents no want of symmetry. There, on the contrary, whilst the right cheek is full, fresh, and rosy, the left cheek is pale and shrunken, like that of an old woman; the skin of the left cheek is thin, corrugated, and may be easily raised; the subcutaneous fatty tissue has almost completely disappeared, so that at several points the integument adheres to the bone; through its thickness the teeth may be felt; the muscles are atrophied and the bones are thinner on the healthy side. The buccinator, the zygomatics, the masseter, and the frontal portion of the temporal, have especially suffered. The muscles of the forehead, the lips, and of the chin, are free; the muscles of the nose are feebly attacked; the ears do not present any difference; a deep furrow ascends from the left commissure of the mouth towards the zygomatic arch and extends as far as the insertion of the temporal muscle. Near to the left commissure of the mouth, under the white and thin skin, a branch of the facial is seen beating. Some rather large veins descend from the suborbital border towards the middle of the face. The skin excoriates easily, but is never covered with sweat, even under the influence of violent exercise. The growth of the eyebrows, the eyelashes, and of the hairs, also the secretion of tears, present no difference on the two sides. Sensibility and motricity are not in any way behind those of the healthy side. Temperature is the same in the two ears (36.8° Centigr.). There is no want of symmetry of the different organs contained in the buccal cavity; no difference of coloration of the gums or of the buccal mucous membrane; lastly, on each side the salivary secretion is the same and temperature at 37.5° Cent. The following measurements give an idea of the changes of size produced by the atrophy:—

	Right.		Left.	
	Inches.	Lines.	Inches.	Lines.
From the extremity of the nose to the lobe of the ear	4	10	4	5
From the same to the outer angle of the eye . .	3	1	2	9
From the middle line of the part to the lobe of the ear	4	½	4	⅜
Thickness of the face	1	½	0	4
Height of the superior maxilla	1	⅜	1	0
" inferior "	1	½	1	⅜

The body of the patient presents otherwise no anomaly. Of eleven cases noted by the author, in five the disease began by a white spot, which by degrees extended and became the seat of depression. In five there was simple pallor of a portion of the integument. In three the affected region subsequently became yellow, turning to bronze as is seen in the cicatrices of certain burns. The skin becomes depressed, not only by the disappearance of the subcutaneous adipose tissue, but by the modification of certain of its elements. To the touch it presents the sensation produced by cicatricial tissue. The muscles become diminished, but continue to contract, after a time the cartilage and the bones are attacked. In consequence of lesion of the cartilage, in one instance the temporo-maxillary articulation became dry and loose. The author relates the case of a young woman in whom a somewhat analogous form of progressive atrophy of the skin attacked the right infra-mammary region. It began as a yellowish spot the size of an almond, which extended in the antero-posterior direction. The skin became hard, depressed and adherent, of a brownish-yellow colour, bluish in certain points, the epidermis shining like that of a cicatrix. Romberg gives this affection the name of "Trophonévrose;" Moore, Virchow, and Hasse consider it a form of progressive muscular atrophy. Drs. Bitot and Lande point out that the muscles retain their contractility for years. They consider it a special autopathic affection, and propose to designate it "Aplasie lamineuse progressive."—*Brit. and For. Med.-Chir. Review*, Oct. 1870, from *Archives Gén. de Méd.*, March, 1870.

34. *Progressive Osteomalacia.*—Dr. Gussmann relates, in the *Würtemb. Med. Corresp.-Blt.*, xl., 1870, the case of a man, 38 years old, an optician, who had

always lived a regular life, and up to the age just mentioned had suffered from no severe disease. On a cold night in winter, whilst in a state of perspiration, he stood for some time with his feet in a puddle of ice-water, and was soon afterwards seized with pain in the neck and back, from which he continued to suffer up to the day of his death, which occurred ten years subsequently. He gradually became emaciated and pale. At length a permanent distortion of the spine set in, with consequent difficulty of breathing, which was mainly due to the compression of the lungs from an enlarged gland situated beneath the sternum. Finally pain was experienced upon the least movement, especially of the trunk. A severe febrile movement now set in from time to time. Perspiration seldom occurred. The urine was generally clear and yellowish. Shortly before death, in consequence of pressure upon the spinal cord by the curvature of the cervical vertebræ, the left arm became paralyzed. A *post-mortem* examination showed that the anterior wall of the chest was very much bulged forward to the level of the third rib, and at the lower portion of the chest, at its sides, the ribs were forced out, very much bent, of a deep bluish colour, their periosteum not thickened, but readily removed, and at some points they were cracked. Upon dividing the integuments they were found reduced to the thinness of paper, and the contents of the cavity of the chest consisted in great measure of a soft fatty reddish substance. The cervical vertebræ were very much curved laterally, with the convexity of the curve to the left side, the dorsal vertebræ were curved outwardly. The vertebræ and bones of pelvis projected backwards. There was some degree of gibbosity of the clavicles and of the sternum, in which the indications of osteo-sarcoma were present. This accords with the previous investigations of Prof. Scüppel on the diseased conditions of the bones. The bones of the extremities, and those of the cranium were in a normal condition. —*Centralblatt f. d. Med. Wissenschaften*, July 23, 1870. D. F. C.

35. *Digitalis in Delirium Tremens.*—Dr. A. WILTSHIRE, records (*Lancet*, Aug. 27th, 1870) five cases of delirium tremens successfully treated by half-ounce doses of tincture of digitalis. He states that Dr. Russell Reynolds, to whom he had communicated these cases, suggested that possibly the good effect of the half-ounce of tincture of digitalis might be owing to the alcohol it contained, and not to the digitalis; and he recommended that, if I had an opportunity of doing so, it would be well to try the effect of half an ounce of proof spirit. Dr. Reynolds, also, suggested that, if I found it necessary to resort to digitalis, it would be desirable to give a watery infusion of the drug, using a dose of it equal in strength to half an ounce of the tincture. It so happened that, shortly after this conversation, Case 3 again came under my care, with another severe attack of delirium tremens. I felt it to be my duty to give him the benefit of ordinary treatment at the outset, as I had done in the first attack for which I attended him; but the result was equally unsatisfactory, and ultimately, I judged it to be necessary to give him digitalis again. But before doing so, I determined to put into practice Dr. Reynold's suggestion. I accordingly gave half an ounce of proof spirit; and although for a considerable time I carefully watched the patient after its administration, I could detect no change whatever either in the pulse, respiration, or mental condition. Finding the result of this experiment to be purely negative, I resorted to the half-ounce dose of tincture of digitalis, as I found a difficulty in procuring good digitalis leaves wherewith to make a watery infusion. This, happily, acted as efficiently as a like dose had done on the first occasion; and the phenomena observed after its administration were of precisely the same character—that is to say, the pulse fell in frequency while it gained in volume and strength, the breathing became more tranquil, and the patient fell asleep within twenty minutes of taking the drug. After some hours' sleep he awoke cured.

No bad or dangerous effects were observed in either of the cases narrated, although, I confess, that the marked decrease in the frequency of the pulse at first made me uneasy; and, had I not recognized that, while it fell in frequency, it gained remarkably both in force and in volume, I should have been alarmed.

It will be observed that my patients were young, or comparatively young, persons; for I am averse to treating the aged by means of large doses of digi-

talis, being, in truth, as it were instinctively, afraid to do so. And, even in the young, I was and still am loth to resort to so powerful, not to say dangerous, a remedy without, in the first instance, having given the patient the benefit of what is regarded as more orthodox treatment, which, undoubtedly, succeeds in the majority of cases.

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36. *Treatment of Scarlet Fever and the Consequent Dropsy by Baths.*—Dr. C. Piltz, gives, in the *Jahrbuch f. Kinderheilk*, N. F. III. 1870, the results of two series of observations. One in regard to the effects of the cold bath in the reduction of the morbidly increased temperature of the skin in scarlet fever, the other showing the result of the employment of the warm bath for the removal of the dropsy consequent upon the disease. The first series were derived from twelve very severe cases of scarlatina, most of them complicated with diphtheritis. These were treated by immersion from eight to ten minutes in a cold bath of 25° C., the surface of the body being at the same time subjected to brisk friction. The bath was repeated at short intervals, until its temperature had risen to 38°.5′—39°.5′, so that it was repeated, in many cases, at intervals, several times in the hour. Of the cases of scarlatina of the same severe character as the twelve in question, in which the bath was not employed, 42 per cent. terminated fatally, whereas all that were treated by the cold bath recovered. The lowering of the temperature of the patient's surface at each immersion in the bath was slight, scarcely 1°. This was probably owing to the continued augmentation of temperature at the invasion and the continuance of the high temperature attained during and immediately after the eruptive stage of the disease. Whatever was the daily variation of temperature in addition to the evening exacerbation, there was always observed to occur a rise also of temperature at 2 o'clock P. M. The pyrogenetic stage of scarlatina occurs with great suddenness. The cause of the slight fall of temperature from the action of the cold bath in scarlet fever, Dr. P. refers to the constant chemical changes the blood is undergoing throughout the organism, in consequence of the morbid increase of respiration, and the inflamed condition of the skin. When, by the cold bath, the temperature of the surface is reduced, a change ensues in the capacity of the peripheral capillaries, causing an increased amount of blood, with an increase of temperature to take place in the more central organs. With the suspension of the bath there takes place a marked determination of blood towards the surface, especially in cases of scarlatina where the peripheral capillary system is much more developed than in the normal state. It is in consequence of the latter circumstance, that upon immersion in the cold bath comparatively, so slight a contraction of the capillaries of the surface takes place; it is also the reason why the pulse and the respiration is less reduced in the more severe cases of scarlet fever than in other forms of febrile disease. In none of the cases of scarlatina treated by the cold bath was any degree of dropsical effusion consequent upon the disease.

The second series of cases were those of dropsy following scarlet fever. They were treated, in accordance with the plan of Liebermeister, by the warm bath of gradually increased temperature, so as to produce copious perspiration. The duration of the patient's immersion was half an hour, the temperature of the bath within that period being gradually raised from 36° to 40° C. The perspiration induced by the bath was kept up for some two hours. After the entire cooling of the surface, the weight of the patient was taken, as a general rule, every second day. In this manner the amount of serous exudation within the abdomen will generally become more or less quickly reduced. The commencement of this diminution of exudation, as indicated by the continued loss of weight of the patient's body, is all important, as most generally, when once commenced, the decrease of the morbidly augmented effusion into the serous cavities and the subcutaneous cellular tissue, goes on without cessation until the normal condition is attained. Occasionally the extent to which the effused fluid is removed is such that the weight of the body is reduced below the normal standard. Before resorting to the warm bath, according to the plan above described, two things must be taken into account. 1st. The existence of disease of the lungs or heart, which is denoted by the presence of severe dyspnœa. Such cases must be care-

fully watched. 2d. The sudden occurrence of extreme loss of weight, so apt to result from the profuse perspiration produced by the action of the bath, especially when the dropsical effusion has remained for a long time unabated under the use of the bath.—*Centralblatt f. d. Med. Wissenschaften*, July 16, 1870.

D. F. C.

37. *Chlorate of Quinia in Pertussis.*—Dr. BREIDENBACK, says, in the *Centralblatt f. d. Med. Wissenschaft.*, July, 1870, that in consequence of the unsatisfactory results he had obtained from the employment of the usual remedies recommended for the treatment of hooping-cough, he felt it his duty to make known the very beneficial results he has found to follow the employment in that disease of the hydrochlorate of quinia, after the recommendation of Professor Binz (*Jahrbuch f. Kinder-Krankheilk.*, I. 3), during a circumscribed but very severe epidemic which prevailed in 1870, at Aeuterhof, near Stotzenbach. The result in all uncomplicated cases of pertussis, and Dr. B. saw no others, was favourable. The remedy must be given in comparatively large doses. Those which Dr. B. gave varied according to the age of the patient—from three weeks to eight years—and the severity of the attack, from gr. 0.1 to 1.0 per diem. No other medicine was given. In some of the cases the children escaped any injurious sequelæ, though, because of the poverty, with which they were surrounded, they were exposed during the winter to a continued spell of cold damp weather. Dr. B. knows of no contra-indication to the use of the hydrochlorate of quinia in cases of pertussis. In his cases it never gave rise to symptoms of intoxication—even, however, when such is the case, all that is required is a reduction of the dose. The beneficial results from the use of the preparation in question in cases of hooping-cough, show themselves with great promptness. In the severest cases, within 48 hours after it has been commenced with, the paroxysms become diminished in violence as well as in frequency. Dr. B. has also seen a commencing and rapidly increasing ecchymosis of the conjunctival membrane of the eye, under the use of the remedy to be promptly resolved. To prevent a return of the disease, Dr. B. continued the use of the remedy in small doses for some time after the prominent symptoms have been quelled.

D. F. C.

38. *Spontaneous Cure of Croupal Bronchitis.*—Dr. ROTH relates, in the *Deutsches Arch. f. Klin. Med.*, VII., 1870, a well-marked case of this. It occurred in an anæmic female, thirty-four years of age; the cure took place without the interposition of any remedial treatment. Physical exploration at the commencement of the disease, and immediately after the discharge by coughing of the croupous membrane, indicated the presence of dry bronchial catarrh (*trocken bronchial katarrh*), with evident respiratory murmur, especially over the region of the left lung, whereas, on the right side the respiratory sounds were diminished.

D. F. C.

39. *Therapeutic Value of some Salts of Potash in reducing or modifying the Amount of Uric Acid in Renal Calculus and Allied Urinary Disorders.*—Dr. W. R. BASHAM in an interesting paper (*Practitioner*, Nov. 1870) remarks that "Empirically and clinically for generations the remedial agency, the lithontriptic virtues, of alkaline preparations in cases of gravel, red sand, or renal calculus, has been universally acknowledged. The interpretation of this action for a long period was fanciful and erroneous according to the prevalence of the doctrines of the humorists or solidists.

"As chemical analysis became more accurate, and the differences in the composition of the several renal and vesical concretions were better known, a simple and apparently satisfactory chemical interpretation of this remedial agency succeeded. It was the conversion of an insoluble organic acid into a soluble salt by an alkaline base, and consequently a more ready excretion in this form."

Dr. B. offers a more complex, but he thinks a more satisfactory interpretation of the action of these alkaline salts; the only one by which the increase of urea and oxalic acid can be accounted for.

"The action of these alkaline salts," he says, "appears to be to facilitate the metamorphosis of uric acid and hasten its conversion into oxaluric acid, which, as oxalurate of ammonia or potash, is excreted in the urine, and which after passing from the bladder is quickly split up into oxalic acid and urea.

"It may be supposed, according to this interpretation, that the alkaline salts given in the uric acid diathesis would promote, if not create, a dangerous tendency to the formation of oxalate of lime concretion, and thus one evil would be exchanged for another. But this is not so. The oxalurate formed with the other ingredients of the urine remains as such while within the urinary organs: it is only when passed and exposed to further oxidation in the air that it eventually breaks up into oxalic acid and urea. The moment the oxalic acid is set free, such is its affinity for lime, that the lime salts of the urine, more particularly the phosphate, yield up a portion of their base to form the insoluble crystalline oxalate of lime. It cannot too forcibly be impressed on the practitioner that oxalate of lime crystals can never be detected in urine received and examined immediately from the bladder; but that the same urine after free exposure to the air will in most cases yield an abundant sediment of them."

He recommends the citrate of potassa to be taken in the following manner and dose:—

"The simplest and most agreeable manner of taking it is to dissolve half a drachm of the bicarbonate of potash in a wine-glass of water, and to add this solution to the juice of a fresh lemon previously squeezed into a tumbler. The citrate thus formed may be taken in a state of effervescence, or after, according to the taste of the patient. It is better, however, to let the greater part of the effervescence subside before taking it; as the escape of any large amount of carbonic acid in the stomach is apt to be disagreeable. This citrate thus formed may be taken night and morning thus, immediately on going to bed and the first thing on rising in the morning, or three times a day if needed. The juice of a fresh lemon is infinitely more agreeable, and I fancy more efficacious than either citric acid or prepared lemon-juice; the latter, however, is preferable to the crystallized citric acid.

"The use of this remedy to accomplish any certain result must be continued night and morning for some weeks. In temporary disturbance of the digestive organs, with a disposition on the part of the patient to suffer from irritation of the urinary passages from crystalline deposit of uric acid, the potash citrate requires to be taken only for a few days; but in cases of gravel and renal calculus, it is only after a steady perseverance in the use of the salt that any remedial influence can be expected."

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40. *Belladonna in arresting Nocturnal Incontinence of Urine.*—The efficacy of belladonna in arresting nocturnal incontinence of urine, so troublesome an affection in children, is probably sufficiently well known. Nevertheless the two following cases related (*Lancet*, Oct. 22, 1870) by Dr. J. B. Yeo, assistant physician to King's College Hospital, are so striking as to be well worth recording. Besides, they occurred at an age which enables us to analyze the mode of the remedy better than could be done in younger patients.

A boy, æt. 16, an out-patient at the hospital, complained of nocturnal incontinence of urine. He was a thin, delicate-looking boy, with a hectic flush on his face; and he manifested in a marked degree that *shamefacedness* and mental distress which so embarrassing an infirmity may well produce. The accident happened every night, and had persisted ever since his infancy. He was ordered to take five minims of the tincture of belladonna, with ten minims of the tincture of the perchloride of iron, three times a day, and to avoid drinking any fluid for some hours before bedtime. After taking this prescription for a week, he reported himself as only slightly better; he had passed urine in bed three or four times since he had taken the medicine. He was ordered to take ten minims of the belladonna tincture instead of five. This reduced the frequency of the accident to first once in seven days; then once in about fourteen days; and, on increasing the dose to fifteen minims, once in three weeks. With the latter dose he took no steel, as his general condition had improved marvellously.

There had been, so far, little or no alteration in the dimensions of the pupils, and vision had been in no wise interfered with; but on the recurrence of a slightly increased frequency of the incontinence, he was ordered twenty minims of the tincture of belladonna three times a day. The nocturnal incontinence now completely ceased, but the vision became so indistinct that he could not "read his copy." He was therefore ordered to take only one dose of twenty minims in the twenty-four hours, and that at bedtime. He continues quite free from a return of the complaint. This patient had never suffered from any irritability of the bladder by day, and had no occasion to pass water more frequently than other people.

The next case was that of a girl, also between sixteen and seventeen years of age, who had similarly suffered ever since her infancy from nocturnal incontinence of urine. She came to King's College Hospital as an out-patient on March 5th last, and at that time she was nightly passing her urine in bed. She was living in the country, and was a fresh-looking, healthy, and robust young woman; but her mental distress and embarrassment were extreme. This wretched malady unfitted her for going into service, might be justly regarded as an impediment to marriage, and altogether made her life a misery. She was put on the same kind of treatment as the preceding case, except that she at once commenced taking ten minims of the tincture of belladonna thrice daily, and she was ordered no steel, as her general condition was good. For the first week little or no improvement was manifested; the dose of belladonna was therefore increased to fifteen minims, which dose she has continued to take ever since. The incontinence rapidly abated, and now scarcely ever occurs. Her appearance and manner have at the same time undergone an entire change; a bright and cheerful aspect has replaced a taciturn, morose, and discontented mien. She has, for nearly a month, been engaged in a situation as a domestic servant, for which she voluntarily applied. Here, then, is a remarkable instance where the whole course and prospects of a life have been rapidly changed by the administration of a few minims of a vegetable tincture!

Dr. Y. concludes that the efficacy of belladonna in these cases is owing to its influence in giving tone to the weakened sphincter vesicæ. This observation is in harmony with the statements which have recently been made as to the action of belladonna as an aperient by promoting the peristaltic contractions of the involuntary muscular fibres of the intestinal canal.

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41. *Incontinence cured by Chloral Hydrate.*—Dr. WILLIAM THOMSON reports (*Lancet*, Nov. 19, 1870) two cases of this. One was in a girl, æt. 12, who suffered for two years with incontinence at night, and frequent desire to pass her urine during daytime. Fifteen grains of the chloral, given at bedtime for two nights, relieved her; and the medicine was then continued in ten-grain doses for a fortnight, at the end of which time she was cured.

The second case was a boy, æt. 13, who had suffered from nocturnal incontinence for seven years. Fifteen grains of chloral given at bedtime at once relieved him; but the medicine was continued for a fortnight, when he was quite well, and continued so when last seen, three weeks afterwards.

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42. *Propagation of Typhoid Fever by Milk.*—Dr. BALLARD, in a paper read before the Association of Medical Officers of Health, pointed out the following facts: Typhoid is, as is now agreed, propagated by sewer poison. Now, there was on the premises of a certain respectable milk-dealer at Islington an underground wooden tank, supplied from the mains of the New River Company. This had not been opened or inspected for years. When opened at Dr. Ballard's desire, some inches of one side were found decayed or destroyed by rats. This allowed the water to flow over into a hole in the ground, evidently scooped out by rats, and leading to certain underground rat-burrows, by which, when the tank was filling, the water flowed into some old brick drains and water-closet drains, close by. Through these underground channels there was free communication between the tank and the sewers; not only could water run away from the tank into the drains, but foul gases from the drains were sure to pass back into the tank; and in June last an alteration was made in the drains, during

which they were obstructed, so that there would be a reflux of the overflow water from these drains and rat-burrows back into the tank. It is a significant proof that this did happen, that at least two families shortly afterwards complained that the milk "had a bad taste," and that the milk when kept became "not merely sour, but stinking." It is certain that the milk-cans were washed out with water from this tank and a shrewd suspicion may exist that some of it was mixed with the milk; but, if so, it must have been done by the men without the master's knowledge; because if ever water was used for this purpose it was taken from another cistern, which also supplied the house, and the tank water was intended only for washing and stable use. Given, then, milk poisoned with a small dose of sewage, we find that the milkman, his family, and servants, and customers were very shortly afterwards affected with a peculiarly fatal typhoid fever; and the connection between the milk as cause, and the fever as effect, is shown by the fact that the fever was confined virtually to those who drank this milk. The medical practitioners of the neighbourhood soon became aware of the existence of an extensive and fatal epidemic in a limited district in the healthiest part of Islington. Of course, all the obvious and usual causes were first suggested, inquired into, and dismissed as untenable. The exposure of new ground in widening the railway, the nuisance of a huge dung-shoot, and the existence of foul drains in houses, were all accused in turn; but it was the milk distribution alone which was proved to be coextensive with the fever. Dr. Ballard ascertained that 142 families, including the milkman's own, formed the "milk walk" or *clientèle* of this dairy. Of these 142, it is known that 70 families were invaded by typhoid in the ten weeks during which the outbreak lasted. In these 70 families were 175 patients, of whom 30 died—a mortality of more than 17 per cent. It was remarkable to see how the typhoid picked out the families supplied from this one dairy, and avoided all the rest. Wherever the fever appeared, it did not attack one, but several members of the household. In certain families, where particular members took no milk, they escaped, whilst others suffered. The women and children, who take most milk, suffered in greatest proportion. A young lady who had a tumbler of this milk daily, whilst the house was supplied from another source, was the only victim in the house. The dairyman himself, and his workmen, who, of course, took the milk freely, were amongst the earliest victims—for this, like other outbreaks, became milder as time went on, and ceased when the business was given up and the cows sold, some time after the death of the proprietor. Meanwhile, the disease was mainly confined to a circle within a quarter of a mile radius from the dairy, whilst the rest of the extensive parish of Islington was virtually free. We have just given the gist of the facts set forth in Dr. Ballard's very able and closely-argued paper, to which we must refer for a fuller narrative. Meanwhile, the notion of *death in the milk-pail* is as disagreeable as it is unexpected.—*Med. Times and Gazette*, Nov. 26, 1870.

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43. *Propagation of Scarlet Fever by Milk.*—Dr. Bell, Professor of Medicine in University of St. Andrew's, details the circumstances attending an outbreak of scarlet fever which was clearly traceable to the use of milk poisoned by the desquamating cuticle given off from the hands of infected persons who were employed to milk the cows of a particular dairy. The facts are shortly these. Between the 9th of May and the end of July, several cases of scarlet fever occurred among the servants and students of a certain educational institution, healthily situated, well built, airy, and isolated in position. At the end of July the students dispersed. No fresh case occurred in the place (St. Andrew's) till the 8th of August, when the servant in a family visiting the place, and who had only been there a week, was seized with the disease. On the 9th, a nurse in another family of visitors sickened, and she and five others in the same house were attacked. Having accidentally heard that the boy who brought milk to the college where the disease first appeared had "peeled off the skin," according to his own statement, at the time of the outbreak, Dr. Bell ascertained the following facts. On May the 1st, the wife of the farmer who supplied the milk returned from a visit to a place where two deaths from scarlet fever had occurred, and fell ill within a few days. She milked the cows. After a while, the boy, whose

occupation it was, also, to milk the cows and to take the milk to town, had the disease lightly, and in due time "peeled." He continued all the time at his work. But now comes the interest of the case. On making out a list of the houses in which scarlet fever had shown itself in St. Andrew's, and asking the dairywoman for a list of the houses which she supplied with milk, with one exception there was a perfect agreement in the lists. In every house to which milk had been sent, scarlet fever had occurred. The cases were twenty-six in all. The attack of the members of the two families of visitors is accounted for by the fact that, when the college broke up at the end of July, the milk thus made available for the use of other customers was sent to the very houses in which the visitors resided. We call the attention of our readers to this important and highly valuable piece of investigation by Dr. Bell.—*British Medical Journal*, Nov. 5, 1870.

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44. *Epidemic of Chorea Minor.*—By Dr. ROGER, one of the most strenuous advocates of the relationship between rheumatism and chorea (see *Gaz. des Hôp.*, 1870, Nos. 65, 66), relates a case of chorea combined with slight articular rheumatism and defect of mitral valve, in which the choreic movements were confined in great measure to the muscles of the right side, while it was on the left side that the rheumatic symptoms were chiefly observed. Dr. STEINER (*Jahrb. f. Kinderheilk.* N. F., III., 1870) witnessed during the first two months of the year 1870, he tells us, a slight epidemic, nineteen cases, of chorea, the production of which could in no case be referred to imitation, inasmuch as no intercourse between the patients had taken place. As the common cause of the disease, in all the cases, Dr. S. adduces the abnormal condition of the weather at the time the epidemic occurred; its general unusual severity, and the unusually sudden and frequently recurring changes in its temperature; in consequence of which any disorder under which children happened to labour passed over into inflammation, the result, as Dr. S. supposes, of an irritation set up in the spine. In only five out of the nineteen cases was it possible to detect with certainty rheumatic symptoms—partly articular and partly endocardiac. The remaining fourteen cases, therefore, must be received as those of a rheumatic affection of the spinal meninges, unassociated with any rheumatic inflammation of the muscles of the joints, or of the heart.

In the treatment of the nineteen cases referred to, Dr. S. found the bromide of potassium to be without effect. Fowler's solution in combination with opium proved, as heretofore, particularly beneficial in its effects, whether in cases attended with great restlessness or with a state of entire quietude.—*Centralblatt f. d. Med. Wissenschftn.*, July 9, 1870. D. F. C.

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45. *Clonic Spasm of the Muscles of the Eustachian Tube.*—D. POLITZER, in the *Wien. Med. Presse*, 1870-XX. relates the case of a girl of 12 years, who had experienced for five months, an almost rhythmical ticking sound in the left ear, that never declined in intensity, and which, during the short periods it ceased, could be voluntarily reproduced. During sleep it was entirely suspended. A hermetically closed manometer tube, containing a drop of coloured fluid, when introduced into the external meatus of the ear, gave no evidence of any movement of the inclosed drop; it was evident, therefore, that the morbid sound in the ear did not result from any abnormal contraction of the tensor tympani muscle. Neither was the ticking sound dependent upon increased arterial pulsation, as it did not correspond in time with the pulsations at the wrist. The cause of the ticking must hence be sought for within the Eustachian tube. On examination a convulsive movement of the left half of the arch of the palate was observed to occur isochronously with the abnormal sound in the left ear. The convulsive movement of the palate ceasing with the cessation of the latter, while the latter was stopped when the velum palati was drawn up and rendered tense during the intonation of the vowels *a*, *e*, *i*, as well as by a voluntary effort of the muscles of the tube. The same has also occurred when by the finger the palate was pressed upwards. The case was evidently, therefore, one of clonic spasm of the muscles of the Eustachian tube. It was cured in a short time by Faradaic electricity.—*Centblt. f. d. Medicinish. Wissenchftn.*, Sept. 17, 1870. D. F. C.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

46. *Erysipelas.*—In the 32d and 33d numbers, 1870, of the Berlin *Klin. Wochenschrift*, Dr. HEYFELDER relates the results of his observations made in the clinical study of 42 cases of erysipelas occurring among the inmates of the military hospital of Tsarskoe-Selo, the soldiers of the guard and others, similar in their antecedents, and in their occupations, clothing, food, etc.

Tsarskoe-Selo is situated upon a tolerably elevated plain, lying between Neva and the Gulf of Finland. The substratum of soil is gravelly, and the neighbourhood rich in forests of pine and other trees. There exists a well-organized and active sanitary police.

From January, 1868, to March, 1870, among 3316 patients there occurred 42 cases of erysipelas; in 28, on the face; in 3, on the face and trunk; and in 11, on the trunk and limbs. In the cases of facial erysipelas, the disease in 12 instances commenced on the nose; in 6, on the ear; and in 5, on a single eyelid.

The individuals found to be most liable to an attack, were those affected with abscesses or furunculi, 13 cases; with angina, 4 cases; with otorrhœa, 1 case; and with wounds, 7 cases. Thus in two-thirds of the cases, the subjects were labouring under some disease or injury of the skin or mucous membrane. In 18 of the cases, the erysipelas was evidently produced by the infected air of the hospital. A close examination of the latter cases has convinced Dr. H. that erysipelas may be propagated from one individual to another by a contagion adherent to the walls of an apartment, to the clothes of physicians and nurses, to the furniture, and to the instruments of the surgeon and obstetrician, or suspended in the atmosphere. Dr. H. has known the disease to be introduced into the wards of the cleanest-kept hospital by a contagion adherent to the thick woollen, and not overclean, garments worn by the field surgeons. He has known, in a new hospital, only occupied in summer, three cases of erysipelas to occur in quick succession, which were traceable to the infection adherent to old bandages, and to the robes used by the surgeons whilst engaged in operating and in making post-mortem examinations. During the cold winter and hot summer months scarcely a case of erysipelas was met with.

In 15 cases the erysipelas appeared in the form of simple erythema; in 16 it was attended with vesication; in 10 with the formation of abscesses; in 3 with chronic glandular infiltration; in 4 with gangrene—twice of the integuments of the eyelids; once of the cheek; once of the patella. In 3 cases the erysipelas was migratory. When the seat of the disease was the face, it was liable to extend to the hairy scalp, or to the neck; seldom to the trunk. Only once was it seen to extend upwards from the body to the head.

Fever was present in every case, preceded by chills. Affection of the brain was seldom present. The duration of the erysipelas of the skin was from 5 to 39 days, or of the entire disease from 14 to 120 days. The simple disappearance of the redness of the skin did not always mark the setting in of convalescence, unless it was attended with a reduction of temperature, from 39 or 41 degrees to 38 or 36; the occurrence of perspiration, and an agreeable sense of ease in the parts in which there had been previously an aggravating feeling of heat and irritation. A return to the normal condition, of all the functions of the organism, was the only reliable indication of entire recovery. Among the 42 cases, three of the patients attacked by erysipelas were syphilitic. The intercurrence of the disease, however, exerted no decided influence upon its cure. Only in two of the 42 cases was there observed a well-marked relapse.

In reference to the treatment of erysipelas, Dr. H. directs that the diseased portions of the skin should be covered, so as to protect them from the action of the atmosphere, by soft linen or cotton bandages, and by anointing with oleum camphorata, while he recommends, internally, calomel, grs. 2, and ol. camphorata, ¼ gr., every two hours; or, where there is nervous depression, spiritus camphoræ 5 drops, from every quarter of an hour to every hour. The patient is to be kept isolated.

Dr. H. lays great weight on the fact, which he believes to be a legitimate deduction from his observations, that erysipelas is always a product of some preceding or simultaneous disease. This fact is often overlooked, and the erysipelas mistaken for a spontaneous affection. The annually recurring epidemic of erysipelas will be found to be developed in patients labouring under recurrent rhinitis, blepharitis, angina, etc, Dr. H. does not believe in the dependence of erysipelas upon a specific morbid change in the blood. The proper erysipelatous symptoms are developed when, in the predisposed, there occurs an augmentation of the local irritation. This irritation he does not, with Volkmann, believe to be of a specific character. The secondary irritation may be: 1st. *Mechanical.* Pressure upon the skin by pads or bandages; retention of matter from the too early closure of the edges of wounds, especially those of the nose or ear, inasmuch as, from the intimate attachment of the skin to the subjacent cartilage, a slight collection of confined pus will produce comparatively severe pressure. 2d. The cause may be *chemical.* The abuse of severe rubefacients; the irritation from abnormal discharges from wounds, ulcers, etc. 3d. The cause may be *physical.* Cold air, especially cold damp air; great heat. 4th. The cause may be an *infection* attached to clothing or furniture, or suspended in the atmosphere. 5th. A *morbid condition of the blood;* as in typhous and puerperal fevers; hydræmia, scorbutus, mercurialismus, pyæmia, gastric disturbance from excess in food or drink, unhealthy dwellings, and insufficient or unwholesome food; insufficient or filthy clothing, as in the case of inmates of ill-conditioned, wrongly-constructed, and badly-managed prisons.

To prevent the occurrence of erysipelas in the wards of hospitals, these should be not only kept perfectly clean, and thoroughly ventilated, but vacated and kept unoccupied for two or three months every other year. In the treatment of erysipelas, a leading object should be to protect the diseased surface from the contact of the atmosphere. The cold water treatment of Volkmann does not appear to be admissible in cold climates. From the local application of nitrate of silver, Dr. H. has not derived any good result.—*Centblt. f. d. Medicinish. Wisschftn.*, Sept. 10, 1870. D. F. C.

47. *Tetanus.*—Dr. OGLE communicated to the Clinical Society of London a case of tetanus in a healthy boy, who got a bruise on his thumb. Three days afterwards he complained of stiff neck, and vomited, and shortly afterwards became affected by opisthotonos. On the fifth day after the injury he was admitted into St. George's Hospital in a state of tetanus. He was put fully under the influence of belladonna; ice was constantly kept applied to the spine, and chloral was given at night to induce sleep. It was noticed that at no time did the sardonic smile exist, and never was there any trismus or (except on one day) difficulty in swallowing liquid food, such as wine, brandy, beef-tea, and beaten-up eggs. In this case the temperature and pulse were registered twice a day; and it was noticeable that almost throughout the patient's stay in the hospital the temperature was higher in the evening than in the morning, on one day reaching 102.3°. About the fourteenth day after the injury the tetanic symptoms began to abate, and by degrees the belladonna and the chloral were discontinued, and also the application of ice to the spine. After about a month from the accident the patient left the hospital quite well, and has so continued ever since. Dr. Ogle suggested that possibly the examination of numbers of cases of tetanus might show that the temperature always increased in the evening, and that this fact might have value in diagnosing true tetanus from certain cases of affections of the spinal cord and its membranes, certain cases of hysteria, and strychnia, and other poisoning. Dr. Ogle believed that the highest temperature arrived at in tetanus was recorded by Wunderlich, who described it as being 108° shortly before death, 112.55° at death, and 113.56° after death. He also alluded to a case of tetanus in which, after the attack, the patient was subject to great irregularity of the heart's action, with much discomfort and palpitation on exertion, as if the mechanism of the organ had been injured in some violent muscular effort.

Mr. CROFT had lately treated a similar case, which got well; he had used hydrate of chloral, and nothing else; its influence was decided. It was inter-

mitted for twelve hours, and the patient got much worse. It kept him asleep. He was about twenty-one days under treatment.

Gopaul Chunder Roy had seen many cases of tetanus in Calcutta. The prognosis depended on the parts affected. If the power of swallowing is lost, they die; if not, however bad, they have a chance of recovery. Nourishment was most necessary. They used opium in the form of smoke, as, used in this fashion, it did not render the patient costive. In one case, belladonna was given; but, constitutional symptoms appearing, it had to be given up. He had seen division of the nerve do good.

Mr. Paget had seen a gentleman three months ago, and to him chloral was given in large doses; it comforted the patient, but did the disease no good. He died just the same. No one, he thought, ever had six cases treated successfully by any one method. He recalled three successful cases. One, a boy, got well—he was kept quiet; another was a man who inhaled enormous quantities of oxygen. In two other cases, oxygen was absolutely inert; a third took chloroform, and got well. Chloroform had been used with others who had died.—*Medical Times and Gazette*, Oct. 22, 1870.

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48. *Strangulated Hernia Reduced by Application of Cold Water.*—Dr. P. Foster reports (*Lancet*, Aug. 27, 1870), two cases of this. In both cases the patients were advanced in life (about sixty), and the hernia had existed in a reducible form for several years. After trying the taxis patiently for about half an hour in each case without producing the slightest effect, I applied cold water. In Mr. W——'s case the tumour disappeared in about ten minutes under the influence of the cold water alone; in Mr. S——'s case, after about the same length of time, the reduction was effected almost immediately upon removing the cold water cloth and reapplying the taxis. The patients were placed in the position usually recommended, and the cold water was applied by means of a pocket-handkerchief, the application being renewed every two or three minutes.

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49. *Rapid Cure of Buboes.*—Dr. J. Grunfeld, assistant to Sigmond, of Vienna, has had much success in extracting the pus by means of a hypodermic needle, India-rubber tube, and syringe. Where the cavity fills again, a second operation of the same kind should be undertaken; and when the pus is unhealthy, weak solutions, either of carbolic acid or chlorate of potash, should be injected, and pumped out again by the same syringe. Such patients as were so treated left the hospital much sooner than those whose buboes had been freely laid open.—*Lancet*, Nov. 12, 1870.

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50. *Treatment of Inflamed Testicle by Puncture.*—At a late meeting of the Medical Society of London (*The Lancet*, Nov. 12, 1870), Mr. Henry Smith made some observations on the excellent results he had obtained from puncture as a means of giving relief to an inflamed testicle. The plan had served him well in about 500 cases. The relief obtained was immediate and permanent, and he believed arose from division to a small extent of the tense and unyielding tunica albuginea. The small amount of blood lost had nothing to do with it.

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51. *Treatment of Strictures by Horsehair Bougies.*—Dr. Mitscherlich, of Berlin, draws attention to the advantage that may sometimes be derived in cases of extremely tight strictures of the urethra from the employment of bougies composed of horsehair. He has always been able to reach the bladder with this simple instrument, and the hair has then served as a means whereby properly conducted sounds, having a minute opening at their extremity, could be made to glide through the stricture. Horsehair, he remarks, is finer than any instrument that can be made of equal strength, whilst it possesses a certain degree of elasticity that renders it much preferable to those composed of whalebone. In nine cases he has had no failure. The following is one of his cases: A soldier was wounded in action in 1849. After some years an attempt was made to extract the ball, which was felt through the soft parts of the perineum. The attempt was unsuccessful, and the result was a fistulous opening

in the perineum, through which a portion of the urine passed, and a hard and very contracted stricture of the urethra. The treatment of this was long essayed with fine elastic bougies in vain; at length one was passed, but the patient absented himself, yet was soon obliged again to submit to treatment. M. Mitscherlich was successful in passing a fine horsehair, and over this perforated sounds of successively larger size, till at length a perfect cure was effected.—*Practitioner*, November, 1870, from *Gaz. Hebdom.*, 1870.

52. *Torsion of Arteries as a Hæmostatic Method.*—Mr. J. D. HILL records (*Lancet*, Nov. 5, 1870) his experience with torsion as a hæmostatic method. He says he has "already practised torsion of arteries in upwards of seventy surgical operations, and without one instance of hemorrhage, secondary or recurrent. The process of healing for the most part has been satisfactory, in some cases complete primary union having occurred where such would have been impossible in the case of the ligature; in others deep-seated adhesion with granulation of the more superficial structures; and, if I may judge from present experience, torsion seems adapted to every kind of operation, whether great or small. As compared with the ligature, its application is more simple and rapid; and should failure occur, it does so on the operating-table. On one occasion only this happened to me.[1] After twisting some atheromatous tibial arteries, one of these arteries gave way just above the twist, and the ordinary ligature was also followed by a like result. Ultimately, however, I secured the degenerated artery (on the principle of acupressure) within a collar of muscular fibres, exerting sufficient compression to arrest the current of blood without breaking the internal tunics.

In the post-mortem examination of arteries which had been submitted to torsion during life, I have discovered no twisting of the internal membrane, as pointed out by Mr. Costello; but simply that incurvation or invagination of the middle and internal coats, so well described by Mr. Bryant's drawings in the 51st volume of the *Medico-Chirurgical Transactions*."

53. *Perineal Lithotrity.*—M. DOLBEAU advocates this as the best mode of removing stone from the bladder, and submits the following facts as proof that the proceeding is entitled to be placed among the recognized operations in surgery. In 1863 he performed his operation for the first time, and the patient rapidly recovered, and was alive and well last August. Since 1863 he has performed the operation on twenty-two patients with but one death. The fatal issue in the single case, the last of the series, may be fairly attributed to other causes than the consequences of the operation, for the patient died, on the fifteenth day after the operation, in an attack of insanity, during which he attempted to hang himself. This case afforded the opportunity of an examination of the parts involved in the operation, and of their exhibition to the Imperial Society of Surgery in Paris.

The steps of M. Dolbeau's operations are:—

1. An incision carried from the border of the anus forwards directly in the middle line, two centimetres (¾ inch) in extent, dividing the skin and subcutaneous tissue.

2. A puncture in the posterior angle of the wound, made to strike a staff in the urethra.

3. Gradual dilatation of the track of the puncture.

4. Dilatation of the neck of the bladder to the entire extent possible with the dilator; that is to say, less than two centimetres in diameter.

5. The crushing of the stone and extraction of numerous fragments.

The dilator used by M. Dolbeau is composed of six blades of steel, which, when closed, form a pointed tube, resembling a porte-crayon; these blades can be separated parallel to each other by a motion of the handle, until the tube

[1] Since writing the above I had occasion to ligature the femoral artery at an amputation of the thigh. Phlebitis (femoral), and probably arteritis, had deprived that vessel of its elasticity, rendering it brittle; all other vessels were twisted.

measures, at the maximum dilatation, nine-tenths of an inch in diameter. The number of blades of the dilator has the effect of enabling it to exercise a circular dilatation perfectly regular in character, and this is strictly confined to the limits of dilatability of the neck of the bladder. The mode of reaching the bladder in this proceeding is the same as in Allerton's operation, except that a mechanical contrivance is substituted for the finger of the operator in dilating the neck of the bladder, a mode of proceeding which M. Dolbeau considers to be too sudden, and liable to the accident of false passage.

The specimen submitted to the Imperial Society by M. Dolbeau showed the parts taken from a patient 67 years of age, who had been operated on fifteen days before death. The bladder was found full of urine, its neck absolutely intact, and the mucous membrane healthy.

M. Dolbeau sums up his communication with the following proposition, deducted from the examination of the pathological specimen:—

1. That it is possible to open a direct passage from the anus to the bladder without damage either to the bulb, the rectum, the neck of the bladder, or the vessels of the region.

2. That it is possible to introduce into the bladder instruments destined to crush a stone and to extract the fragments, without rupturing the neck of the bladder, or without destroying the power of retention of urine.

It is to be regretted that M. Dolbeau's communication does not contain the details of the twenty-one successful cases, especially as regards the size of the stones removed and the ages of the patients.—*Dublin Quart. Journal of Med. Science*, August, 1870, from *Gazette des Hôpitaux*, No. 26, January, 1870.

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54. *Fracture of Anterior Spinous Process of Ilium by Muscular Contraction.*—Drs. S. Joy and J. W. McWhinnie report (*Canada Med. Journal*, Sept. 1870) a remarkable case of this in a very muscular youth, æt. 17, who in a foot-race, in turning, at a given point, felt something snap in his right hip, walked a few steps and fell. "On examination distinct motion and crepitus could be felt by pressure over the process, also by placing the thumb over the origin of the Sartorius and rotating the thigh. The fracture extended into the notch below, but there was no great tendency to displacement, save when the leg was abducted, thus placing the Sartorius upon the stretch, the process, doubtless, being partially kept in place by the fibres of the tensor vaginæ femoris arising from this process on the one hand, and Poupart's ligament on the other, when tension was taken off the Sartorius. The patient was placed in bed with the thigh flexed and the shoulders raised, a bandage being applied to aid in steadying the fracture. It may be as well to state that this position and adduction of the right leg was maintained by bands attached to the posts of the bed. In two weeks the patient made a good recovery without displacement.

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55. *Skin-grafting.*—Mr. Pollock read particulars and exhibited to the Clinical Society of London (Nov. 11, 1870), several cases of skin-grafting and skin-transplantation. In relating the particulars of the first case in which the skin-grafting had been attempted in this country, he stated that in 1869 M. Reverdin originated in Paris this method of treating large ulcerated surfaces. In May, 1870, the author first heard of M. Reverdin's experiments, and at once decided to test the treatment. A girl, eight years of age, had been in St. George's Hospital for some three months and a half, with an extensive open burn of the right thigh, of more than two years' duration. The ulcerated surface extended from the buttock to the knee—broad above, and ending almost in a point below. Mr. Pollock at first transplanted two small pieces of skin about the size of millet seeds, taken from the lower part of the abdomen. Subsequently three, and again other pieces, were transplanted at various periods—in all, about fourteen pieces had been transplanted. The child was exhibited to the members of the Society, and it was seen that this extensive burn was nearly healed, in a period little over five months, without any perceptible contraction of the cicatricial tissue originated by the transplanted skin. The child had greatly improved in health as the progress of cicatrization had advanced. In this case two pieces of black skin had been on one occasion transplanted to the ulcerated surface,

and became attached; when increased in size, the area of the pigment deposit had considerably increased in one of them, although the whole of the cicatricial tissue due to the transplantation of this portion of skin was not generally dark coloured. The sore was attacked some time after by sloughing, which was chiefly confined to the portion in which the black skin had been engrafted, and unfortunately destroyed the whole of the cicatrix due to this trasplantation. Mr. Pollock made some general remarks with respect to the mode of transplantation, and the conditions requisite to the success of the operation. He usually transplanted very small pieces, similar to the plan pursued by M. Reverdin, and considered it essential to the success of the operation that the surface of the granulations should be in a healthy state. In some cases the operation had entirely failed, in consequence of the state of the sore. In other cases, though the piece transplanted had become attached and vitalized, yet owing to the state of the patient's health, it had remained stationary, and gave no sign of increase. Mr. Pollock, in conclusion, thought a tribute of admiration and gratitude was due to M. Reverdin from the profession for the boon he had conferred upon surgery by the introduction of this original method of dealing with large and obstinate ulcers.

Mr. LAWSON read a paper "On the Successful Transplantation of Portions of Skin for the Closure of large Granulating Surfaces." He exhibited two patients in whom this mode of treatment had been attended with satisfactory results, and related the history of a third who had been equally benefited by the method of transplantation. In one patient a large ulcer of the leg, which had resisted all treatment for over four years, was completely closed in a few weeks after a piece of skin the size of a fourpenny-piece had been planted on it. As soon as the new skin had established its vitality, granulations sprang from the circumference, and rapidly closed-in the wound. In another patient the results were equally satisfactory. In a third case Mr. Lawson formed a new eyelid for a patient who had a complete ectropion of the upper lid. He dissected the lid from its attachments, pared at two points the corresponding tarsal margins, and united them by two fine sutures, and thus obtained a fixed level surface upon which to transplant a portion of skin. The parts were then left, and on the fourth day, when the wound was covered with healthy granulations, he transplanted a piece of skin of the size of a threepenny-piece, and two days later another portion, of the size of a silver fourpenny. Both pieces rapidly united to the granulating surface, and the space between them was speedily filled up with new cicatricial tissue. A new lid was thus formed, which was sufficient to protect the eye from exposure; but the presence of two pieces of skin, different in appearance to the ordinary eyelid integument, gave to the patient a peculiar and rather unsightly look. In each of these patients the skin which was engrafted not only soon became vascular, but acquired sensibility, and after ten or twelve days could appreciate the slightest touch with a blunt instrument. The conditions which Mr. Lawson found essential for the operation were: 1. That the new skin should be applied to a healthy granulating surface. 2. That skin *only* should be transplanted, special care being taken that no fat adhered to it. 3. That the portion of skin should be accurately applied to the granulating surface. 4. That the new skin should be kept in position without interruption, and that it should be lightly covered with a layer of lint, and over that a small compress of cotton wool, and a bandage, for the purpose of maintaining its warmth, and thus to assist in retaining its vitality until it had established its new life.

Mr. CROFT mentioned a case in which he had transplanted a piece of skin a quarter of an inch square on to a large granulating sore of the leg. Eleven days afterwards it had doubled in size, and was now progressing very quickly towards recovery.

Mr. FRANCIS MASON remarked that the size of the pieces which were transplanted was worthy of special consideration; that, according to his own experience, pressure was important, and he regarded several failures that had occurred as due to the difficulty of obtaining this condition satisfactorily, on account of the situation of the parts treated.

Mr. CARTER asked Mr. Lawson if a better result might not have been obtained

by transplanting a small piece from the other eyelid, so as to preserve the natural delicacy of the skin of the parts.

Mr. GANT agreed that the similarity of texture on both sides of the body was worth recollecting in these cases.

Mr. SPENCER WATSON gave brief particulars of cases that had come under his notice at the Great Northern Hospital, and considered that the success of the operation depended very much in keeping the transplanted pieces in position.

Mr. HENRY ARNOTT spoke of a case in which two ulcers existed, one having been treated by transplantation and the other left alone; both closed pretty much at the same time; and, in reply to Mr. Heath, said that the case was one of keloid, following a burn.

Mr. REEVES thought that the large pieces of skin should be transplanted.

Mr. ARTHUR DURHAM eulogized Mr. Pollock and the operation, and said that in a case of pigmentary nævus that had come under his care it was positively beautiful to see the islands of skin joining the mainland and the processes of repair proceeding so rapidly.

Mr. CALLENDER quoted cases under the care of Mr. Pick and Mr. Willet, all of which had done well; and in reply to a question from Mr. Heath, said in all the cases cited, the ulcer was, as far as he knew, of the usual tertiary syphilitic form.

Mr. POLLOCK closed a long discussion by briefly explaining his own views as to the physiological aspect of the question, and as to the success of the operation in syphilis, and regretted that the state of Paris had prevented any recent intercommunication with Dr. Reverdin, the talented author of the operation. According to his own experience, and particularly with rupial sores, small pieces of skin answered as well as large; and, in this matter, some consideration should be shown to the patient.—*Lancet*, Nov. 19, 1870.

56. *New Instrument for securing the Pedicle in the Operation of Ovariotomy.*—Dr. GRAILY HEWITT, adopting the conclusion that the best method of treating the pedicle in ovariotomy is to bring it to the surface of the wound, suggests a new method of securing it in that position. A framework of steel, shaped somewhat like a shoe-buckle, measuring two inches and a half by one inch and three-quarters, the steel band being two-eighths of an inch wide and one-eighth of an inch thick, is provided with studs, eight in number, fixed on the framework. The studs project three-eighths of an inch. The pedicle is to be tied in two or three segments, according to its size, by double twine ligatures, and the ends fixed to the studs of the framework, the ligatures acting much as the tongue of a buckle. Thus the pedicle can be easily maintained at the surface of the wound, easily treated, and the healing materially expedited. —*British Med. Journal*, Oct. 1, 1870.

57. *Ovariotomy in a Girl aged Twelve.*—M. JOUON, professor in the medical school of Nantes, successfully performed this operation a short time ago. The patient had been suffering from an abdominal tumour for the last eighteen months, and the growth had reached the size of a twin pregnancy at the end of gestation. The diagnosis was not easy, and at one time hydatid of the liver was suspected. On the strength of the last supposition it was determined to open the abdomen by caustics. When the slough had been incised, it was found, after the evacuation of some liquid, that the abdomen contained a capacious pouch, studded with fibrous tumours projecting in its cavity. As now the diagnosis was clear, and the girl lost ground every day, ovariotomy was performed. The cyst was multilocular, and the adhesions numerous; but, in spite of these difficulties, and the patient's tender age, she was well on the forty-sixth day after the operation.—*Edin. Med. Journal*, November, 1870.

58. *Solvents for Ear Wax.*—The *Revue de Thérapeutique*, June 15th, 1870, contains an account of some experiments instituted by Dr. J. E. PETREQUIN, Ex-Surgeon-in-Chief of the Hôtel-Dieu of Lyons, to ascertain the best solvent for ear wax. Inspissated wax is well known to be a common cause of deafness,

and it is very desirable to determine the most efficacious and least painful means of removing it. Dr. P. tried successively almond oil, a mixture of glycerine and oil, pure glycerine, alcohol, a mixture of olive oil and essence of turpentine, alkaline solutions of white and also of green soap in water, pure turpentine, chloroform, and sulphate of carbon. He found some to have no solvent properties, and others too irritating. The best solvent, he states, is tepid water, which acts equally on soft and indurated cerumen. The hard cerumen under the action of water swells, has a tendency to disintegrate, and is very easily broken up.

OPHTHALMOLOGY.

59. *Does Disease of the Cerebellum lead to Loss of Sight?*—The difference betwixt a tumour and softening from embolism, with regard to the production of certain secondary symptoms, is very great. So far as Dr. HUGHLINGS JACKSON has observed, double optic neuritis never occurs along with softening from embolism of the middle cerebral artery, but it nearly always occurs with tumours—they are usually syphilitic—in the region which this vessel supplies. It is one of the results of a local encephalitis. It is sometimes discussed whether disease of the cerebellum produces loss of sight or not. The evidence is that it sometimes does and sometimes does not. The word "disease" is vaguely used. Softening rarely if ever does—tumour nearly always does. (Cases of tumour so placed, or so large, as to lead to pressure on the corpora quadrigemina, are excluded from consideration.) In more general terms, *destruction* of the cerebellum does not, but irritation (encephalitis about a foreign body, or irritation propagated in vaso-motor regions?) does. Then the word "produce" is used vaguely also. The existence of blindness along with disease of the cerebellum is no proof that the disease has destroyed any "centre for sight." The blindness depends on inflammation of the optic nerves. This is not a mere verbal refinement. It is quite certain that there may be very abnormal ophthalmoscopical appearances when the patient can read the smallest type, and when he considers his sight to be quite good. Moreover, blindness may not follow such inflammation. The nerve-fibres of the optic nerve-bundle are not in *fault;* they *suffer* from the faults of the other ingredients of the bundle. The inflammation, Dr. Hughlings Jackson believes, is brought about by frequent and irregular contractions of the arteries supplying the nerve-trunks, the contractions being the result of discharges of unstable gray matter in the part of the brain diseased. (Sudden *attacks* of temporary loss, or of great defect, of sight are not very uncommon in the onset and course of cases of optic neuritis in physician's practice.) In considering the manner of production of changes of nutrition through the intermediation of vaso-motor nerves, we have not to think merely of persistent contraction of arteries as the result of a continued irritation of the vaso-motor nerves, nor merely of permanent dilatation, the result of palsy of these nerves. For nervous discharges—of gray matter, at least—are intermittent; and if, in cases of tumour, much gray matter be involved in the local and secondary encephalitis, there will be not only excessive but many repeated discharges. Thus orderly rhythmical nutrition of the nerves will be impossible. With regard to the position of disease producing optic neuritis (and other symptoms of its class, *i.e.*, secondary symptoms, the results of a local encephalitis), the question whether white or gray matter is involved is of more importance than the question whether the cerebrum or the cerebellum be the seat of disease. Admitting, as is most probable, that the optic nerves have connections—probably very indirect connections—with the cerebellum, as they have with the cerebrum, it is still maintainable that destruction of great part of one lobe of the cerebrum or of the cerebellum produces no defect of sight. But nervous organs are connected not only by nerve fibres, but also by arteries (indirectly by vaso-motor nerves). Hence, whilst *destruction* of no part of the cerebrum or cerebellum leads to

loss, nor even to defect, of sight, irritation by tumour in any part of either may lead to inflammation of the optic nerves, on which inflammation blindness usually, not always, follow.—*Brit. Med. Journ.*, Oct. 14, 1870.

60. *Parenchymatous Keratitis associated with Acute Rheumatism.*—Mr. Spencer Watson read before the Clinical Society of London (Oct. 14) the reports of four cases of this. The patients were all young adults, all under 19 years of age. Both corneæ were affected, and the symptoms and appearances were not distinguishable from those of parenchymatous keratitis due to inherited syphilis. Out of a series of cases, extending over the last ten years, Mr. Watson had only observed acute rheumatism in association with parenchymatous keratitis in these four; but he suggested that in some others of the series this complication may have itself cut short the observations, by preventing the attendance of the patients at the Ophthalmic Hospital. From these cases it was conjectured that the interstitial keratitis of inherited syphilis may be allied to rheumatic inflammation, and may possibly yield to treatment such as is beneficial in the latter disease. Two of the patients, whose cases were related, were exhibited to the Society. In one, the attack of keratitis had occurred eight years ago. It was found in this case that the young woman had become myopic since, and most probably in consequence of the attack.—*Brit. Med. Journ.*, Oct. 22, 1870.

MIDWIFERY.

61. *Application of Pressure to the Uterus in Cases of Lingering Labour.*—Von Ritgen, in 1856, suggested the employment of external pressure on the uterus as an adjuvant in cases of powerless labour, and the suggestion was put into practice by Kristeller, in 1867, and he published a number of cases in which he had advantageously resorted to it.

Dr. W. S. Playfair in an instructive article (*Lancet*, Oct. 1, 1870) observes "It is certain that the advantages to be derived from external pressure are not yet widely known or recognized; and as I have now received very material assistance from it in many cases of lingering and powerless labour, I believe it may not be without interest to state briefly the result of my experience on this point, especially as I do not know of any published cases in this country in which its use has been described.

"The class of cases in which external pressure is likely to prove serviceable is of very frequent occurrence, viz., in which the presentation is natural, and the pelvis roomy, but in which delivery is retarded simply from deficiency or absence of uterine contraction. These are the cases in which resort to the forceps is so often essential, in which the head has passed well into the pelvis, possibly descended as low as the perineum, and in which apparently but one or two good pains are required to complete the delivery.

"Firm pressure, applied under such circumstances, may act in two ways: First, and most commonly, it may merely stimulate the sluggish uterus to increased exertion, just as firm pressure after delivery will cause a relaxed uterus to contract. In this way, pains that are feeble and ineffective may be rendered strong and useful, and a natural termination may result when artificial assistance might otherwise be required. I have of late been frequently in the habit of thus stimulating the uterus, and I feel certain that I have in many instances greatly shortened the progress of a labour that threatened to be long and tedious. It is, indeed, often curious to observe how rapidly the pains increase in force and duration, under the stimulation of gentle and steady pressure at the commencement of each pain." * * * "I need hardly add, by way of caution, that gentle but firm pressure in a proper direction is to be used and that all rough handling of the uterus is to be avoided. The pressure can be most readily applied with the patient lying on her back, but this is by

no means essential, and I have constantly used it in the ordinary position on the side, and without disturbing the patient."

62. *Quadruplets.*—Dr. J. W. MILLER records (*Edinburgh Med. Journ.*, Oct. 1870,) a case of this. He was called at 2 P. M., June 6, 1870, to a woman in her fifth labour, and who was then in about the eighth month of her pregnancy. She was very big and had considerable œdema of the lower extremities. "At 2.30 the os was almost fully dilated and the membranes protruding. The pains were languid, and after a dose of ergot, at 3.15, I ruptured the membranes. The head presented. A male child was quickly expelled at 3.30. I had barely time to tie the cord and remove the child, when a second bag of membranes protruded at the vulva. These having been ruptured, the breech of a second child was found presenting. I extracted it easily, about three minutes after the first; it also was a male. A third bag of membranes came almost immediately outside the vulva, and burst spontaneously with a great gush of liquor amnii. The feet presented, and the third child, a female, was extracted about five minutes after the birth of the second. On passing the hand over the abdomen the uterus was found still large, and examination revealed a fourth bag of membranes, which I ruptured. The head presented. The uterine contractions being very feeble, I administered another dose of ergot, with about a glass of whiskey, but the head continued to make very slow progress. The patient being rather exhausted, and moreover somewhat panic-stricken under the very extraordinary nature of the event, I sent for the forceps, and with one blade over the occiput scooped out the head. This child was born at 4.10, about half an hour after the third; the delivery of the four children thus being completed in about forty minutes. The first three were born in a fairly vigorous condition; the fourth was dead. One of the placentæ was single, and came away almost immediately; the other three were in one large mass, and were extracted about ten minutes after the birth of the last child. The uterus contracted well, and there was no hemorrhage. The patient's pulse was 70, and of fair strength. The female child died during the ensuing night. At my visit next day one of the others seemed to be dead, revived a little towards evening, but died during the night. The other, and last, died next day. I was allowed to weigh two of the children, and found each to weigh about 3¼ lbs.; the four together would weigh about 13 lbs.

"The mother for a few days had considerable œdema of the face and legs, but this quickly disappeared, and she made an excellent recovery."

63. *Rupture of Uterus—Gastrotomy.*—Dr. J. H. TYLECOTE records (*Lancet*, Nov. 5, 1870) a case of rupture of the uterus. The child escaped into the abdominal cavity; gastrotomy was performed, and a still-born child removed. The mother recovered.

64. *Inversion of the Uterus.*—Mr. COWARD read before the Obstetrical Society of London (Nov. 2, 1870), a case of this in a primipara, æt. 22. The labour was normal, and the inversion took place suddenly when only gentle traction was being made on the cord. The inversion was at once reduced, the placenta having been first removed. The patient made a good recovery.

Another case was reported at the same time by Dr. MADGE, also in a primipara, where labour was quite natural. After delivery, the nurse was making firm pressure on the uterus, when inversion suddenly took place. Dr. Madge first peeled off the placenta, and then reduced the uterus without much difficulty. The patient made an excellent recovery.—Dr. BRAXTON HICKS said that, in the cases of inversion which he had seen, there had been no traction by the medical man on the cord. In one case the patient had not been touched, as the placenta had been expelled spontaneously before the attendant's arrival; and in another, it occurred immediately on the delivery of the child by the forceps, in a case of inertia of the uterus. He thought that a short funis in many cases would be sufficient to commence the action, if the fundus were flaccid.—Dr. HALL DAVIS believed that two classes of cases of inversion of the uterus occurred in connection with labour. First, there were the cases resulting from

traction on the umbilical cord, however applied, the placenta being as yet attached to the fundus of a relaxed uterus; as during the completion of the second stage of labour, the advancing child dragging on a short cord, or during the sudden birth of the child, the parturient woman being erect. Secondly, there were cases in which the displacement was the effect of an inverted action of the muscular fibres of the uterus, quite independent of, and often in the absence of, the attendant. The latter class of cases he believed to be the most frequent.—*British Medical Journal*, Nov. 19, 1870.

65. *Treatment of Puerperal Convulsions by Chloroform and Barnes's Dilators.*—Dr. A. Wiltshire maintains (*Practitioner*, Nov. 1870,) that in chloroform we have an agent capable of arresting and keeping in check the hideous convulsions which might of themselves cause death; and in Barnes's air-bags a simple and harmless means of rapidly dilating the maternal passages and securing a speedy termination of labour.

66. *Puerperal Fever.*—Dr. T. Hulbertma, Professor of Medicine in the University of Utrecht, in a preliminary paper which appeared in the *Centralblatt f. d. Med. Wissenschaften*, July 9, 1870, remarks that, although the conviction has within a recent period become more fully confirmed as to the non-specific character of childbed-fever, still, in every recent work on obstetrics, puerperal fever is set down as a specific disease of the newly-confined female. While the term puerperal fever is most generally used in this restricted sense, by some it is applied to nearly all the febrile affections which occur during the puerperal period, hence it is important that, when the term is made use of, the actual disease intended should be clearly defined. In some of the so-called cases of puerperal fever observed by Dr. H., it was not possible to detect any difference between the pathological phenomena they exhibited, and those which occasionally occur subsequent to surgical operations, and in cases of traumatic diphtheria. Such conclusion is not, however, in accordance with those which others have drawn from their observations (Buhl and others); these confine puerperal fever, like milk fever, strictly to the lying-in female, and remove it entirely from the field of surgical pathology. Dr. H. remarks that, with Billroth, he should include, as fevers liable to occur in the puerperal period, and which have been, and still are, to a great extent, included in the books, under the general term puerperal fever, namely, traumatic diphtheritis (*endometritis diphtheritica*), phlegmonous inflammation of the womb (*parametritis*), peritonitis, lymphangiotitis, phlebitis, simple traumatic fever, pyæmia, and septicæmia. We have then, according to Dr. H., up to the present time, no evidence that there is any form of disease, occurring during the puerperal state, which, in respect to its etiology, its symptoms, and its pathology, differs from traumatic diphtheria, and the other occasional non-specific pathological results of parturition.

D. F. C.

67. *Cæsarean Operation performed four times in the Subject.*—Dr. Oettler reports a curious case of this. The subject of it had a narrow pelvis, resulting from rickets, with which she had been affected from infancy. The first labour was in 1853, when she was 23 years of age, and her subsequent ones in 1857, 1859, and 1863. Each time she was delivered by Cæsarean section of a living and viable child, rapidly recovered from each operation, and at the date of the report was in good health.—*Revue de Thérapeutique*, 15 Sept. 1870.

68. *Diet of Parturient Women.*—Dr. Hugh Miller calls attention to the very vague instructions given by obstetric writers on this subject. Particulars of a case were given, in which careful nourishing diet given during utero-gestation enabled the patient in her last confinement to escape suffering from uterine inertia. From an examination into the physiology of the changes in the uterus and breast, Dr. Miller believed that the fat-cells existing in abundance in the milk during the first few weeks were due to the changes in the womb after parturition; that the disintegrating uterus was broken up into fat-cells, which were absorbed by the blood, and through the circulation were se-

creted by the mammary glands. Hence a heat-forming diet was neither necessary nor was indicated, and at times might be positively injurious; whereas a flesh-forming diet, by maintaining the strength, enabled the woman to make up for the waste of tissue during labour, gave her support, and maintained the vigour of her body while the further changes were going on. The author had found great benefit through selecting the parturient woman's diet from as nearly as possible the kind of food which she was in the daily habit of taking, giving it in a liquid form and in diminished quantity. The advantages in adopting a nourishing diet to the mother he believed to be; 1. Maintaining her muscular strength; 2. Avoiding irritation to the mammary glands and enabling her to suckle sooner; 3. Securing a quicker and better recovery.

Dr. ROBERT BARNES says that he has noticed great mischief brought about by giving nutritious diet too soon after parturition. He did not say that such diet was not necessary; but there was a prevalent tendency to go too far, and to load the stomach before the patient was able to bear it. The system after parturition required repose, and that in consequence of the changes that took place little food was at first required. It was not desirable to give stimulants at all, and certainly not solid food.—*British Med. Journal*, Oct. 1, 1870.

69. *Functional Hemiplegia in Child-bearing Women.*—Dr. T. CLIFFORD ALLBUTT, in a paper read before the British Medical Association (August, 1870) calls attention to some cases which have occurred in his practice of late, which seem, he says, to constitute a uniform clinical group, to have a serious aspect, and to be curable by appropriate means.

The patients in all his cases "were women, and in all but one the complaint followed parturition, or shortly preceded it. In the one other case it occurred with a menorrhagic condition. The symptoms in all were curiously alike. Hemiplegia, on the left side, was the prominent symptom in each, hemiplegia in which weakness was always present and was often considerable. The patients speak of it, however, rather as 'numbness,' by which they do not mean so much loss of tactile sensibility in any precise sense, as rather a sense of dragging and heaviness in the limbs, attended with more or less constant tingling. This tingling, again, is commonly associated with some neuralgia, sometimes with severe neuralgia, and the well-known infra-mammary pain often forms part of it. Some general debility is usually present, and some apprehension; the former being perhaps an essential part of the state of things, the latter being due to alarm at the anticipated palsy. The tongue may be coated, or it may be red and irritable; usually, however, it is clean and flabby, clean with the smoothness of diminished activity. The stomach is feeble; it calls for no food, and digests that which is taken with difficulty, and with flatulent distension. The bowels likewise are sluggish or morbidly irritable."

Such cases Dr. A. thinks cannot be rare, as he has notes of eight well-marked cases which have occurred in his practice during the last three years.

"The pathology of this functional hemiplegia is obscure. It has in my experience always affected child-bearing women; but I suppose only because this is the commonest exhausting condition in women.[1] It is perhaps most common after parturition, when the exhaustion of suckling is added to the effects of gestation. Dr. Handfield Jones seems, however, to have seen it in men [Functional Nervous Diseases]; and I think I have seen something like it in women not bearing children, but liable to uterine discharges of a sanguineous or leucorrhœal kind. The left side alone has been affected in my cases, but probably we shall find that the right side is likewise subject to the affection, but less frequently and in less degree.

"I have reserved for this part of my paper the fact that in three of my cases I found the axillary temperature lower by an average of .8° to 1° (C.) on the affected side. In one case I found no marked difference in the two sides, and

[1] Mr. Horsfall, of Leeds, tells me of a similar case of left hemiplegia, apparently functional only, and which was completely cured. The patient, however, was a maiden lady of advanced years, and the condition may have been some other than that which I have attempted to describe.

in the remaining cases the temperature was not noted. This fact points to some diminution in the blood-supply of the parts, a diminution which can scarcely be accounted for by the loss of exercise, and which is probably due to vaso-motor irritation. It is farther to be noted that the numbness of the limbs is often attended with neuralgia, and not only with neuralgia of the same parts, but also with neuralgia in the infra-mammary region. We know that intercostal neuralgia is often associated with vaso-motor disturbances, with strange visceral sensations, with flushings, 'heats and chills,' and the like; and this, I believe, is to be explained by the close association of the dorsal nerves with the sympathetic nerves." * * * "I am disposed to regard the affection entirely as one of the visceral and vasal nerves, the lower cervical and upper dorsal branches receiving irritation from the visceral branches, and the nerves of motion and sensation being indirectly affected by the want of due blood-supply. Be this as it may, my purpose is fulfilled if I have drawn attention to a common clinical group of symptoms, and have shown that the affection is a less serious one than it appears. At the same time it tends to relapse, and a long continuance of the affection with times of increased severity may really bring the patient into the state of a paralytic, if a thorough system of treatment be not adopted. The treatment must be as follows: If the tongue be coated and the bowels irregular, an alkali, with tincture of nux vomica and tincture of rhubarb, and a light bitter must be given three times a day, with a nightly pill of aloes, belladonna, and capsicum; but if the tongue be clean and flabby, the mineral acids must replace the alkali. As soon as the tongue is improved in appearance and the bowels are regulated, steel in combination with ammonia or ether must be given with meals, or immediately after them, and a draught containing liquor strychniæ must be given between meals. This distribution of the medicine is important, for the prescription is thus more efficacious than when the steel and strychnia are given together. Pepsine must be liberally prescribed with the flesh-food, to insure its complete digestion, and the meals must be small, light, and numerous. It will probably be needful to order some light nutriment at four or five o'clock in the morning, or before rising from bed. No alcohol is necessary, and I prefer to avoid it, unless a little better ale or sherry decidedly improve the appetite at dinner. The pills will probably need repetition either nightly or at short intervals. As soon as these remedies are well established, that is in two or three days or so, cod-liver oil must be added, but it must be given once a day only, and in small quantities, say from two to four teaspoonfuls. If cod-liver oil be ill borne, rum and milk may replace it. All exercise should be short of fatigue, driving in the open air is best. Galvanism may be an useful adjunct, but I doubt it; faradisation is actually injurious."—*British Med. Journal*, Oct. 1, 1870.

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70. *Connection between Inflammatory Conditions of the Uterus and its Displacements.*—Dr. J. Henry Bennet read an interesting paper on this subject before the Midwifery Section of the British Medical Association, August, 1870.

The following propositions express his views on the subject:—

"I consider that, under the influence of mechanical doctrines pushed to an extreme, uterine displacements are by many too much studied *per se*, independently of the inflammatory and other lesions that complicate and often occasion them.

"2. That the examinations made to ascertain the existence of inflammatory complications are often not made with sufficient care and minuteness, as evidenced by the fact that I constantly see cases in practice in which inflammatory lesions have been neglected entirely, and in which the secondary displacements have been alone studied and treated.

"3. That inflammatory lesions are often the principal cause of uterine displacement through the enlargement and increased weight of the uterus or of a portion of its tissues which it occasions.

"4. That when such inflammatory conditions do exist, as a rule they should be treated and cured, and then time should be given to nature to absorb and reduce hypertrophied and engorged tissues before mechanical means of treatment are resorted to.

"5. That the relief from the sensation of bearing down which pessaries and bandages give is no real criterion of their being the proper means to use, such relief being often felt when there are inflammatory lesions present, which their presence aggravates.

"6. The above statements must not be considered in any way to imply that I do not recognize other causes of displacement of a non-inflammatory nature, such as laxity of ligaments and soft parts, wide pelvis, laceration of perineum, severe shocks," etc.—*British Medical Journal*, Oct. 1, 1870.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

71. *Toxicological Observations on Frogs deprived of Blood.*—These experiments were instituted by Dr. LEWISSON, of Berlin, in order to determine how far neurotic poisons act directly on the nervous system, or on the other hand only indirectly by acting primarily on some of the chemical constituents of the blood, or the blood-corpuscles. He takes advantage of the fact pointed out by Cohnheim, that in frogs a solution of common salt can be injected into the vessels, so as nearly completely to wash out the blood. The process of injection at a uniform pressure is continued, with short intermissions, for 1½ hours, and the result is that the frog has a fluid circulating, composed essentially of salt-water, with very few red-corpuscles and rather more white ones; such frogs Cohnheim has named *salt-frogs* (salz-frösche). In animals so treated the heart continues vigorously beating, and the power of motion is to a considerable extent retained. Now if neurotic poisons, administered to such frogs, act at all, they must do so directly on the nervous system, and not by changing any of the constituents of the blood. The conclusion he comes to is, that all the neurotics whose action on frogs is rapid have a direct action on the nervous system. That is to say, such neurotics as morphia, chloroform, etc., which produce their action rapidly in normal frogs, were found to have a similar action in the salt frogs, and these must, therefore, act directly on the nervous system. On the other hand, such bodies as hydrogen, carbonic acid, and carbonic oxide, which may act on the frog for hours without result, may possibly produce their effects by a simple poisoning of the blood.—*Glasgow Med. Journal*, November, 1870, from REICHERT and DU BOIS-REYMOND'S *Archiv für Anat. und Phys.*, etc., No. II., 1870.

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72. *Poisoning by Carbolic Acid.*—Dr. A. WILTSHIRE, records (*Medical Times and Gazette*, Oct. 22, 1870), a case of this in a lad æt. 19, suffering from enteric fever, to whom a tablespoonful of strong carbolic acid was given by mistake. A condition of collapse rapidly came on; the patient never rallied, and died two days after. During life there was marked lividity of the skin, which presented a mottled appearance. The stools were of a blackish-brown colour; and the urine presented the appearance which usually follows the exhibition of carbolic acid, viz., a very dark brown or blackish colour.

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73. *Essence of Turpentine as an Antidote to Poisoning by Phosphorus.*—Dr. P. SPILLMAN has given (*Archives Générales*, Sept. 1870,) a *résumé* of the most recent experiences in regard to this. He quotes four cases in which patients had taken large doses of phosphorus; one recorded by M. ANDARET (*Bull. Gén. de Thérap.*, 1869), two by M. L. SORBETS (*Gaz. des Hôp.* 1869, No. 65), and one by KŒHLER (*Berliner Klinische Wochensc.*, No. 1, 1870); in all which turpentine given in emulsion entirely relieved the distressing symptoms, and recovery took place. He also refers to the experimental researches of M. PERSONNE (*Comptes-rendus*, 1869), who infers, from his experiments on dogs, that the turpentine acts by preventing the phosphorus which has penetrated into the blood from being oxidized at the expense of the oxygen of this fluid.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

A Case of Strangulated Hernia of the Tunica Vaginalis of rare Variety; Operation; Gangrene; Death. Reported by GURDON BUCK, M. D., Surgeon to New York Hospital, St. Luke's Hospital, &c. &c.

Wm. McNulty, æt. 43, truckman, of robust constitution, residing No. 217 West 16th Street, while handling a heavy case of merchandise, aided by an assistant, had the entire support of it thrown upon him by his assistant giving out. He was conscious at the moment that his strength was taxed beyond its utmost capacity. This happened about three weeks previous to my visit; and two or three days after this occurred a swelling appeared on his right groin, that subsequently showed itself also in the scrotum and involved the right testis. He did not suffer sufficient inconvenience to interrupt his usual occupations. The swelling would partially disappear on lying down. On Sunday, Oct. 16th, at 3 P. M., I visited Mr. McN. at the request of, and in consultation with his attending physician, Dr. Henry McClain, and found that on the Friday preceding, while at his work, McNulty was suddenly seized with severe colic pains soon accompanied with vomiting which obliged him to give up, and which have both persisted to the present time. He has had no fecal evacuation since his seizure, though laxative enemata and other means have been resorted to. . . The vomited matter is represented as stercoraceous and stains his linen greenish yellow. The abdominal pain, which is very severe, is referred to the umbilical region. The belly is supple and but little distended. The countenance is calm, the pulse 82, and the respiration tranquil. The right testis hangs low in the scrotum, is more than double the size of its fellow; it retains its flattened, elongated form, and is of firm, solid consistence. The scrotum in contact with it is reddened and somewhat adherent to its anterior surface. From the upper part of the testis, the cord can be traced up to, and into the inguinal canal, and is of the size of the little finger, firm and cylindrical in form. The whole appearance was that of orchitis; the testis being also tender and painful under pressure. Besides the scrotal tumour, there was another distinct, well-defined, and rather prominent swelling over the inguinal canal and external abdominal ring. The skin covering it was unchanged in colour and supple. The subjacent parts constituting the swelling were supple and movable on each other, and nothing could be felt of a firm resisting character such as a strangulated hernial tumour would present.

The coincidence of what appeared to be orchitis with marked symptoms of strangulated intestine, and the equivocal condition of a second tumour in the groin occupying the ordinary situation of an inguinal hernia as it emerges from the abdominal ring, were conditions exceedingly perplexing in reference to diagnosis. The scrotal tumour itself isolated from the inguinal tumour by the intervening cord, appeared to have no connection with the hernia, and yet regarded as a coincident orchitis its origin lacked a

satisfactory explanation. It was, however, decided in view of the unequivocal symptoms of intestinal strangulation to perform an explorative operation notwithstanding the inguinal swelling itself presented nothing that could be regarded as incarcerated bowel. An incision was made over the inguinal swelling, commencing a short distance above it, and continuing downward and inward over the course of the inguinal canal a distance of about three inches. The subcutaneous tissues were successively divided, and the external ring exposed spanning and tightly constricting the parts emerging from it. Guided by the forefinger nail insinuated under the edge of the ring, Cooper's Hernia Bistoury was passed up into the inguinal canal, and divided the constriction. Other constricting bands situated higher up were also divided and the finger carried on beyond the internal ring, but without arriving in the open peritoneal cavity. The enlarged cord could be traced up to the abdominal cavity, but no strangulated intestine could be distinguished. Attention was now directed to the parts constituting the inguinal swelling. A sac appearing to cover and inclose these parts, an attempt was made to open and follow up its continuity into the peritoneal cavity above. Successive delicate layers were divided and fatty masses agglomerated together and resembling omentum were exposed, but no cavity could be distinguished. In this very perplexing state of things, Professor H. B. Sands was sent for and joined our consultation. After a critical examination of the parts Dr. Sands also shared our perplexity. Directing our attention again more particularly to the enlarged cord, a feeling of elasticity was observed which determined us to puncture its sheath. A director was then inserted through the puncture, and passed readily upward within the sheath, and arrived in the open wound high up. Dividing the sheath upon the director, to our great surprise, we found it inclosing a portion of intestine. Continuing the division of the sheath downward to the scrotal tumour, we exposed a noose of small intestine occupying the cavity of the tunica vaginalis, and lying in contact with the testis which was of normal size and condition. Withdrawing the incarcerated noose, a well-defined gangrenous spot of a grayish-brown colour, and of the size of a copper cent piece was found occupying the convex surface of the gut where it was doubled upon itself. The seat of strangulation had evidently been at the junction of the tubular sheath of the cord with the sac proper of the tunica vaginalis. Following the track of the open sheath upward, the finger arrived in the cavity of the peritoneum where it now moved freely in every direction. The hernia was now reduced with its gangrenous portion left out at the external ring, where it was secured by four opposite sutures inserted through the sound intestine above the line of demarcation bordering the slough and the corresponding edges of the wound. The gangrenous part was laid open to allow a free escape of the fecal contents of the bowel. Warm flaxseed poultices were directed to be applied to the abdomen, and special attention to cleanliness was enjoined. A pill of one grain of opium was ordered to be administered every two hours.

Oct. 17, nine o'clock A. M., patient comfortable; had vomited only once; pulse 76, soft.

18*th.* At 12 o'clock patient is progressing favourably; pulse 68, full and soft; complains of flatus. A free discharge of fecal fluid is taking place from the groin. None from the anus. Suspend opium and give per anum an enema of infusion of catnip; apply cakes of oakum wrung out in hot water to the groin, and renew them every two hours.

19*th*. Patient continued comfortable as above reported till 4 o'clock this morning, when a change for the worse took place. He was seized with a chill, and acute abdominal pain and vomiting. His pulse rose to 120 and over, and became very small. The abdomen became tumid, and the fecal discharge from the groin ceased. Great restlessness also supervened.

At 11 o'clock in the evening, though quieted and somnolent from the effect of enemata of sol. sulph. morph., his condition is worse; all his bad symptoms persist. His pulse is very weak, his countenance anxious, his hands cold and clammy; in fact he is in a state of collapse—in which, however, he survived till the afternoon of the next day at 6 o'clock when he expired. No post-mortem examination was made.

The very remarkable and rare feature of this case consists in the fact that the knuckle of small intestine, in its descent along the cord into the scrotum, instead of distending the tunica vaginalis immediately after emerging from the external abdominal ring into a sac continuous with the pouch proper that lodges the testis, dilated it only into a tubular canal of uniform calibre throughout its entire length, which resembled very exactly an enlarged spermatic cord, such as is commonly met with in acute epididymitis. Hence the perplexity of the diagnosis. The works of the standard authors on hernia furnish no similar example.

No. 46 West 29th Street, New York, Dec. 16, 1870.

A Case of Asymmetry of the Two Halves of the Body. By J. C. Hubbard, M. D., of Ashtabula, Ohio.

Master C. S., a native of this town, nine years of age; height 49⅝ inches; ordinarily active and intelligent. His father is of "Scotch-Irish" descent, rather under size, a family characteristic. The father inherits from maternal side some of the peculiar Celtic features. The boy's mother is rather above middle size, and has a brother and sister who are unusually tall and stout. Her blood relatives are large-sized people. She presents no marks of Celtic origin. About five months after the birth of this child, his parents noticed that his right hand, foot, and ear were larger than those of the opposite side. My attention was called to his peculiarities before he was a year old. We found the same disproportions existing then as are now presented; the deformity is undoubtedly congenital; there is but little if any difference in the size of the sides of the head, face, and jaws; none in the teeth, mouth, and fauces, excepting the tongue; the right half of this organ is notably longer and thicker, and constitutes rather more than two-thirds of the tip. When it is protruded it curves away to the left as though that half were paralyzed. The right eye, ear, and testicle are the larger. On examining the penis in its flaccid state it appeared symmetrical, but soon erection occurred, and the superiority in size of the right corpus cavernosum was found to be remarkable. The penis curves to the left, so as to present a striking deformity. The right half of the glans constitutes two-thirds of the whole. His gait is limping and very awkward. Active exercise produces undue weariness in the muscles about the right hip. The left hand and foot, aside from their disparity in size, are more delicate in shape than their fellows of the opposite side. The relative length of the forefinger to the stump and of the fingers to each other, the outlines of the wrist and palm, lead me to think that the

type of left hand (and foot too) *is* Celtic, though the larger members of the opposite side are decidedly Saxon. The following are partial measurements of the two sides:—

	Right Side.	Left Side.
Around shoulder-joint	10½ in.	9½ in.
" elbow-joint	7⅜ "	6⅝ "
" wrist-joint	5¼ "	4⅞ "
From acromion process of scapula to styloid process of ulna	15½ "	14⅝ "
" ant. sup. spin. proc. of ilium to int. malleolus	25⅞ "	24⅝ "
" heel to tip of big toe	8⅝ "	7⅝ "
Around calf	10 "	9⅜ "
" instep	8¼ "	8 "
Length of ear	2⅝ "	2 "
Breadth of ear	1⅛ "	1 "
Around right half of chest	12¾ "	
Circumference of chest at level of nipple		23½ "

Bromide of Potassium to prevent the Nausea following the Use of Opium. By J. Y. DALE, M. D., of Lemont, Pa.

In May, 1870, I was called to see an old lady who was suffering with lumbago. The pain was excruciating, and the least attempt at movement caused intense agony. Not having my hypodermic syringe with me, I gave her the third of a grain of morphia by the stomach. She recognized the medicine by its taste, and said that the sickness it caused was almost as bad as the pain; for on several previous occasions she had taken morphia, and each time the nausea was very severe, and continued for a week. I prescribed smaller doses to be given at intervals during the night, if necessary, in connection with other treatment, and on my return next morning found the pain much mitigated. She begged for something to prevent the nausea, and I could think of nothing that would be so likely to have this effect as bromide of potassium; so, by way of testing its virtues in this respect, I prescribed ten grains to be taken every three hours, with directions to continue the morphia in one-eighth of a grain doses if the pain were severe enough to require it. Convalescence was rapid; in two days my patient was able to walk, and not a single unpleasant symptom manifested itself. I have since used the bromide where there was the same idiosyncrasy in regard to opium, with similar good results.

[This observation is confirmatory of that made by Dr. DA COSTA. See number of this Journal for April, 1870, p. 365.]

Quinia in Croup. By ARMISTEAD PETER, M. D., of Georgetown, D. C.

In *The American Journal of the Medical Sciences* for October, 1870, there is a short article on "Treatment of Croup," by Dr. Fordyce Baker, taken from the *American Journal of Obstetrics*, May, 1870. At the suggestion of Dr. H. Magruder, of this town (a gentleman who has had an extensive practice for the last forty years), I commenced the use of quinia in croup about eight years ago. Dr. Magruder informs me he has used it for the *last twenty years*, and has never lost a single case where he has seen the patient *before the membrane formed*. From my limited experience I must say I have been much pleased with the therapeutic effect of quinia. In prescribing quinia for croup, I expect nearly the same result as if giving it for a well-marked ague, especially where the disease is of an adynamic character. I administer it during the day in broken doses, so as to prevent the recurrence of the night symptoms. Of course I have used, as does Dr. M. *whenever indicated and necessary*, emetics, hot-water cloths to throat, foot-baths,

calomel, opium, belladonna, stimulants, and the alkalies; but most of these adjuvants are for the same purpose as purgative and febrile mixtures in chills. I believe quinia to be decidedly beneficial in the majority of cases of what are called simple, spasmodic, or pseudo-membranous laryngitis.

Dislocation of the Femur on the Dorsum Ilii of five and a half Months' Standing reduced by Manipulation. By J. H. McKee, M. D., of Bannack City, Montana.

Mrs. B., æt. 27, of moderate general health, while travelling with her husband and two little children from Bannack to Salmon City, Idaho, one hundred and ten miles, was thrown a considerable distance from an overturning wagon going down a steep hill and alighted upon her left knee on solid ground. Not feeling herself injured, so sudden was the shock, she attempted to rise to rescue her infant then lying some ten feet distant, but when half erect she fell back helpless, and concluded she had badly sprained her limb. Hoping it might be restored by the use of liniments, &c., no examination was made, and they proceeded on their journey yet forty miles, the patient all the while suffering the most excruciating pain. There being no physician or surgeon in Salmon, or at the time nearer than Virginia City, Montana, one hundred and eighty miles, it was determined to fully test the virtues of external applications, which were persevered in for about five weeks, when they returned to Bannack to procure surgical aid. A doctor was called from Virginia City who detected a dislocation; *guessed* there was a fracture somewhere in the hip-joint, pronounced that nothing could be done, applied a long splint extending from the knee to the crest of the ilium, and left the patient to make the best of her misfortune. So matters remained; the patient returning to her home till the early part of August, when I, having recently located in Bannack, was consulted.

I found the patient quite emaciated, not only from great suffering with the limb, but from a chronic cystitis of many years' standing, which was especially troublesome and painful during pregnancy. She was also in the third month of pregnancy; and stated that for several months before the accident, and during the latter months of her last pregnancy, she felt a disposition in her left hip to slip, insomuch that she was at times helpless and compelled to keep her bed, thus proving that either the socket was very shallow or the ligaments within and around it were very weak. The left limb was two and a half inches shorter than its fellow, and dangled powerless like a dead member. The head of the femur could be easily felt high upon the dorsum ilii, the patella facing the internal condyle of the opposite knee, and the toes quite advanced and hugging the instep of the opposite foot.

In view of all these discouraging features, the long standing of the dislocation, and no professional counsel, I was somewhat at a loss to determine what to do. The patient's life was one of torture, increased at the time by her cystitis and pregnancy; and while the procuring of abortion was regarded a desperate remedy, it was nevertheless considered, and the idea abandoned. Upon a consultation with the family and friends, and a full explanation of the nature and uncertainty of success in the operation, I decided to attempt reduction by manipulation.

Accordingly on the 10th of August, having selected as the anæsthetic equal parts of chloroform and alcohol, anæsthesia to be maintained by ether, and having prepared the patient on a low couch with pelvis firmly fixed by a T-shaped system of padded straps fastened to the floor and side

rails of the bed, I cautiously administered the chloroform to full insensibility, and then grasping the ankle with my left hand, with the bend of my right arm under the knee, giving me perfect control of the limb, I slowly flexed the thigh inward and upon the abdomen, to dislodge and throw out the head of the femur and relax the Y ligament, and then with a circular abduction over the abdomen, brought the thigh to a perpendicular and right angle with the body with slight rotation and sudden and forcible traction in the direction of and against the socket, and then down alongside its fellow. Comparison showing it too short, the same movements were repeated, but this time flexing the thigh less, and making firmer traction perpendicularly toward the socket; this was followed by an *audible snap* announcing the completion of the work, and then bringing down the limb and comparing it with its fellow, I found it to mate it in every particular. To secure it in this position and prevent a luxation on a reaction and contraction of the muscles of the hip-joint, the knee slightly everted was firmly bound to the bed-rail until the muscles and ligaments concerned had become reconciled to the new location of the head of the femur. The operation occupied but *one minute*, and was performed without the aid of any person or machinery.

The patient gained strength in the hip slowly on account of her feeble health, until, at the expiration of about two weeks, by some imprudent movement on her part, she slipped the hip, but it was easily replaced again, the head of the bone taking its place with an audible snap.

The patient's condition now continued to slowly improve, but her pregnancy advancing so rapidly, the predisposition of the hip-joints to slip during this state, formerly so annoying, began to be felt in the right hip, rendering her almost helpless, besides impeding the recovery of the wounded hip, and indubitable evidence having been furnished of the extreme shallowness of the socket, and of the great relaxation of the ilio-femoral or Y ligament, it was found impossible without the use of the angular splint as recommended by Bigelow (which in her condition could not be applied) to hold the femur steadily in its place; for in defiance of all efforts the femur was disposed to settle upon the lower edge, or just below the socket as the patient lay upon her back. The limb, however, remains the exact mate of its fellow, and foot everted to the same degree as the other, while strength and use of the limb are gradually regained, so that now, October 1st, she moves about on crutches; bears considerable weight upon the foot, and will eventually find it a useful limb, but not much so until after her confinement.

Intra-Uterine Medicator and Sponge-Tent Carrier. By J. STOCKTON HOUGH, M. D., of Philadelphia. (With a wood-cut.)

As intra-uterine medication is now receiving a considerable share of the attention of gynæcologists, it occurred to me that a *more thorough* and *less dangerous* method than that of injection was desirable. That there are objections to the treatment by injection of a fluid into the uterine cavity, we have only to refer to the recent medical literature to be convinced.

The first objection is its failure to accomplish the desired result; the second is the resulting peritonitis, set up by the entrance of the injected fluid into the abdominal cavity through the Fallopian tubes.

Again, medication by injection is not thoroughly efficacious, because the *mucus* on the diseased mucous membrane is tenacious, and cannot be detached and washed away by the injected current of water as well as by

the sponge of a mop. The author has, therefore, been led to devise the instrument described and figured below.

The instrument consists of a solid cylindrical bar sliding in a slotted tube—the bar projecting one and one-half inch beyond the shoulder of the tube at one end, and having a ring for the index finger at the other, where it moves in the slot. The projection of the bar is intended to carry a sponge-tent, when it is desired to introduce one into the uterus—which is discharged by retracting the bar. The ring at the extremity is intended to receive the thumb of the right hand, and the other ring the index finger of the same hand.

To medicate the cavity of the uterus with a fluid contained in an absorbent material, such as a sponge, there are two indications to be fulfilled: first, the *easy introduction* of the sponge into the os uteri, and second, the *discharge* of the medicated *liquid* after the sponge has been properly introduced. These indications are met by the following described contrivance:—

The sponge-tent carrier as above described has adapted to it three steel wires, which compress the sponge and hold it in position for easy introduction. To adjust the sponge, you proceed as follows: A rectangular piece of sponge of sufficient thickness is *wrapped around* the central stem, when it is ready for the triangular adjuster, shown in the cut, an arm of which is passed along the side of the cylinder of sponge and penetrates the hole in the end of the tube—thus *baling* the sponge on the stem. These wires, on account of the compression, are buried in the sponge and do not come in contact with the tissues at all. The sponge (which should be of the finest quality) is now trimmed with a pair of scissors, until it assumes a perfectly cylindrical form, when it is ready for use.

The sponge is dipped into a medicated solution and introduced into the dilated os uteri, and the rings at the extremity are made to approach each other by means of the index finger and thumb of the right hand, when the cylinder of sponge will be shortened in length and *increased* in *diameter*, which will force the medicated fluid to the periphery of the cylinder; a part of it being discharged in the uterine cavity, and much remaining on the sponge, all of which is brought into actual contact with the uterine mucous membrane when the instrument is rotated, the viscid mucus being swept away at the same time.

The instrument is neatly made by Mr. Gemrig, of German silver, silver-plated.

August 1, 1870.

DOMESTIC SUMMARY.

Intra-Uterine Injections.—In a discussion on the use of these applications, at a meeting of the Medical Society of the County of New York (May 23d, 1870), Dr. T. Gaillard Thomas made (*New York Medical Journal*, August,

1870) the following highly important, and, in our estimation, very just remarks:—

"My impression is that intra-uterine injections do not constitute an advance in the treatment of uterine diseases; that they have done, and are going to do, a great deal of harm; and that, though they are popular, their evil results will cause them, after a more thorough trial, to be discarded.

"In making such an assertion, I must, of course, sustain it by proof. I commenced the use of intra-uterine applications, both solid and fluid, at least ten years ago. At that time I was extremely fond of them, and treated many a case as endometritis of the body which I should now treat as cervical endometritis. One method which I very often employed was that brought forward by Dr. Lente, of dipping a probe into the fused nitrate of silver, and passing it up to the fundus uteri and around the cavity, so as to cauterize its whole surface. Once, after I had made such an application of the solid nitrate, the patient became dangerously ill with the symptoms of cholera morbus, due entirely to this cause. A year or two later, a lady, who had been for some time under my care for corporeal metritis, and endometritis, became discouraged, and at my suggestion consulted one of the best gynæcologists of this city. I saw her again at the end of a week, when she had a phlegmon the size of my fist, in one broad ligament. The inflammation had been set up by the introduction of an ointment of calomel through the canula of Dr. Lente.

"I have mentioned these cases where solid applications to the cavity of the uterine body have done injury merely to illustrate the fact that solid applications, which are less harmful than fluid ones, are, when made to this locality, not free from danger. Let us see how the matter stands with the injection of fluids. Dr. Nott has said that the entrance of any injected fluid into the peritoneal cavity cannot produce a serious effect immediately, but time must first elapse for inflammatory action to become established. In this statement I think the doctor is in error. A case which I saw some years ago in company with Dr. Peters, whom I see present to-night, bears upon this point. The lady was known to have simple ovarian tumour, I think about as large as the fist. Called to her in the night, I found her almost in collapse, saying she was sure the tumour had broken. She was right, and her terrible prostration was due entirely to the entrance of the fluid of this benign ovarian cyst into the peritoneum. I recall the case of one of my students, who in lifting a trunk felt something give way, was at once violently collapsed, and finally died of peritonitis. The cause was in all probability a rupture of the vermiform appendix.

"My own impression is that, where intra-uterine injection is practised, a certain number of the cases will die from penetration of the fluid through the Fallopian tubes. This certainly does produce the most intense agony, and the most sudden and fearful collapse. I remember that, while making such an injection, by means of a syringe with a screw piston, each turn of which forced out a single drop of fluid, when I had thrown but five drops into the uterine cavity, the patient exclaimed—'My God, doctor, you have killed me.' Her sudden pain and terror seemed to me due to the passage of the fluid into the peritoneum—nothing else could have been quick enough. She went through a tedious course of pelvic peritonitis, barely escaping with her life.

"But it will be said that in these cases the cervix was not dilated, and that, had it been so, no evil would have resulted. I reply that the very dilatation, so coolly talked of theoretically, is itself attended by danger. Within a fortnight past I have had one of the most melancholy cases I have ever met in practice. The lady had come to me, three months before, with anteversion of the uterus. I had treated her with an anteversion pessary, and now considered that part of the treatment completed. So I told her, 'I am to-day going to dilate the neck of your womb, and to do nothing else.' I did this at my office, using three of Dr. Peaslee's dilators; and she left for her home at Staten Island, promising to write to me. This was on Monday of last week. Tuesday evening I was summoned to Staten Island, where I found the lady with violent pelvic peritonitis, of which she died on Sunday evening. I think the mischief was done by the dilatation of the neck of the uterus. Yet I am very sure that the

dilators were passed with requisite skill; for I spend much of my time in this practice, and this operation is one to which I have constant resort.

"Some time since a gentleman wrote me, from St. Paul, Minnesota, that he had known a case where a small amount of a solution of iodine was injected into the cavity of the uterus, and the woman died suddenly of peritonitis.

"Now, these are selected cases I allow, and it is by no means a final argument against any procedure that such selected cases should speak badly for it. The question is very different, however, from that in such a capital operation as amputation of the neck of the uterus, for example, which you enter upon expecting, and the patient and her friends expecting, that there is at least a chance of a fatal issue. Such an operation is justifiable and often imperative, as offering the only chance for life, though it may be a very small one. But if the woman be suffering simply from some slightly annoying affection, she does not expect to die from the routine procedure you resort to for its relief; and you have no right to employ any means attended with such hazard.

"Again, I see no necessity for intra-uterine injections. My impression is that the uterus rarely contains over a drachm of fluid, and that it is an error to represent it as often containing a considerable amount of putrescent discharge. But, if it have within it such fluid requiring removal, dilate the cervix, and replace the organ, if ante- or retro-verted, or flexed, and what is to hinder the fluid from coming out? The dilatation itself will accomplish much. Then, in addition, if necessary, give ergotine, or ergot, and general tonics, and, if required, introduce the cotton-wrapped probe. Every one knows the difficulty of removing the plug of viscid mucus from the cervical canal. For this purpose I keep a supply of little bits of sponge, not larger than my finger's end, which, being wet and squeezed, will wipe the canal nicely, and then be thrown away. This done I see no difficulty in passing up the probe armed with cotton, and painting over the whole internal surface.

"For myself, I never use intra-uterine injections, even for the hemorrhage of abortion. If we know anything about the pathology of uterine hemorrhage, we know that it should be stopped by closure of the vessels from contraction of the tissue of the uterus itself. After delivery we all recognize that the only proper thing to do is to empty out the clots and stimulate the uterus to contraction. And, even after abortion, I look upon the injection of styptics as a bad plan. Take, as an example, a case which came before me a few days ago. A woman had been bleeding for three months after an abortion. It seemed to me there must be something in the uterus to cause the flow; for, otherwise, what is there in an abortion to keep up hemorrhage for such a length of time? In such a case you may take it for granted that something has been left behind. The doctor in charge of the case consented to my dilating the cervix and introducing the curette. I took away a number of little bulbous bodies, remains of the placenta—those little granulations which bleed if you look at them, and pour out blood profusely if you touch them with a probe. The next day the hemorrhage had completely stopped, and within a week the patient's general condition was greatly improved. I mention this not as a case important in itself, but as a type of many where success may be attained by a comparatively minor procedure—for I esteem the use of the curette as a far less grave operation than the injection of the uterine cavity.

"I wish it to be distinctly understood that I have been objecting not to intra-uterine medication, but such means as those mentioned by Dr. Peaslee—the brush or probe, for instance—but the special form of medication by injections; and I include even the injection of warm water, which I think can hardly ever be necessary. * * *

"My views are the result of careful thought, and I hold them with great strength of conviction. I feel that we shall soon see such unequivocal bad results from intra-uterine injection, that the profession will be forced to abandon it as a customary practice."

Extract of Belladonna in Cholera Morbus.—Dr. W. E. Whitehead, Ass't Surg. U. S. A., extols (*Pacific Med. and Surg. Journ.*, Oct. 1870) the efficacy of the solid extract of belladonna in cholera morbus. It has been, he states,

his "habit to order from the third to one-half of a grain of the solid extract of belladonna, made into as small a pill as possible—this sized pill to be taken every four hours; to be continued till the toxical effects of the medicine manifest themselves, or till relief comes, which latter is generally the case long before the former are manifested."

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Amputation at the Knee-Joint.—Dr. JOS. F. MONTGOMERY, of Sacramento, records (*Pacific Med. and Surg. Journ.*, Oct. 1870) a case in which he performed this operation in March, 1864. The resulting stump, he states, "is smooth and well covered with original or natural integument, as the edges of the cuts finally united closely and firmly, and it bears pressure well. The patient wears a plain wooden leg, with a socket, cushioned with prepared sponge, covered with soft leather, in which the stump fits—the weight of the body being borne upon it without causing abrasion or soreness, and with perfect comfort. A gum-elastic cushion, ingeniously fixed with a spring to the lower end of the leg, prevents any noise or jar in walking; and, with the aid of a cane, the patient walks rapidly and easily in attending to his business. The movements of the thigh are very free in every direction, as in the perfect limb—the muscles and their attachments, save the ligament of the patella, being intact. The patella itself is retracted on the front of the thigh about two and a half inches, where it is as movable laterally as it was in its natural position, the attachment at its lower end being firm and unyielding."

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Excision of Os Calcis.—Dr. HUNTER MCGUIRE records (*Medical Times*, Oct. 1, 1870) two cases of caries of the os calcis in which he excised that bone. In both, the tendo Achillis, he says, "formed a new insertion, and acted in raising the heel. Probably it became attached to the dense callous fibrous or osseous tissue which filled up the cavity left after the removal of the bone."

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Failure of Vision, from Disease of the Retina, as a Symptom of Bright's Disease.— Dr. JOHN GREEN, of St. Louis, reports (*St. Louis Medical and Surgical Journal*, July, 1870) four cases of Bright's disease, in which the changes in the retina as revealed by the ophthalmoscopic examination first aroused suspicion of renal trouble.

Dr. Green remarks that the one constant symptom of the amblyopia of Bright's disease "is failure of vision, which may be so slight as scarcely to attract notice, or so great as to incapacitate the patient from guiding himself. This may appear at any stage of the disease, and may increase and diminish or even almost entirely disappear after having reached nearly total blindness, while the fatal malady is steadily marching onward to its inevitable termination. It is absolutely painless in all its stages, and is marked by no external sign of inflammation. Occasionally, as in case 4, in an elderly person with otherwise normal eyes it is very liable to be mistaken for incipient cataract, on account of the general similarity of the subjective signs in the two affections. In case 1, the coexistence of serious head symptoms naturally suggested the diagnosis of amaurosis from cerebral disease. Cases 2 and 3 seemed to point to local lesions of the eye, in the one instance to commencing sympathetic ophthalmitis, and in the other to choroidal and retinal changes dependent on the distension of the eyeball incident to progressive myopia.

"The diagnosis of albuminuric retinitis by the ophthalmoscope is usually a matter of great simplicity. The principal source of possible mistake lies in the close resemblance which some cases of this disease bear to infiltration of the disk of the optic nerve and the retina immediately around it, occurring simultaneously in both eyes from obstructed venous circulation dependent on intracranial pressure from effusion, etc. Very recently, too, a case has been most carefully studied and reported by Dr. H. D. Noyes, of New York, in which the ophthalmoscopic appearances were absolutely identical with those which belong to albuminuric retinitis, but in which the disease was unmistakably not Bright's disease but *diabetes*.[1] This case, taken in connection with the microscopical

[1] Transactions of the American Ophthalmological Society, Fifth Annual Meeting, 1868, p. 71.

investigation of the retinal changes in a case of diabetes observed by Treitz and reported by Arlt,[1] leaves scarcely a doubt of the identity of the retinal affection, and goes far, therefore, to confirm the few earlier but somewhat defective reports of retinitis associated with glycosuria.

"The important fact for the general practitioner of medicine, to be deduced from the study of cases like those now reported, is that simple failure of vision, of whatever grade, and especially when it occurs simultaneously or nearly so in the two eyes, may be a symptom of grave renal disease which has not yet revealed itself by other marked signs. The chemical and microscopical examination of the urine becomes, therefore, an imperative duty in this class of cases in which we should, *a priori*, expect least from it."

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Syphilis of the Nervous System.—Dr. E. L. Keyes, from a clinical study (*New York Med. Journ.*, Nov. 1870) of 34 cases of syphilis of the nervous system, deduces the following conclusions:—

1. That nervous symptoms depending upon syphilis may arise within the first few weeks after an infecting chancre, or at any period later during the life of the individual.

2. That it is presumable, from the study of published autopsies, that the earlier a nervous smyptom (paralytic or otherwise) occurs, the less likely is there to be any material lesion which an autopsy can reveal; and that in a given case there exists no constancy of relation between the nature, the situation, and the severity of the lesion, and the nature, situation, and severity of the nervous symptom to which that lesion may give rise.

3. That cerebral congestion is probably the pathology of many of the earlier nervous syphilitic symptoms.

4. That syphilitic hemiplegia occurs, as a rule, without loss of consciousness, even when the attack is sudden; but that the paralysis usually comes on gradually, the patient being under forty years of age, and having had fixed constant headache for some time before the attack.

5. That mydriasis, existing alone, or with other nervous symptoms, without positive disease of the eye, is presumptive evidence of syphilis.

6. That paralyses of single muscles, or sets of muscles, are frequently syphilitic.

7. That syphilitic paraplegia generally comes on gradually, often without any local symptom to call the patient's attention to the injured portion of the cord, and that it is rarely complete. That the bladder almost always suffers more or less, and calls for special local treatment. That paraplegia may be developed as a symptom of inherited syphilis.

8. That syphilitic epilepsy usually occurs after thirty, in patients who have not had epilepsy in early life. That headache is liable to precede the attacks. That the convulsions occur often, many in quick succession, the intermission between the series of attacks being comparatively long, but that, during this period, headache or other nervous symptoms exist and become aggravated, contrary to what obtains in idiopathic epilepsy. That syphilitic epilepsy is liable to be associated with, or followed by, some form of paralysis.

9. That aphasia is often associated with the intellectual disturbances caused by syphilis.

10. That loss of memory is a common nervous symptom of syphilis, as are also all forms of mental disturbance—from mild hallucinations and illusions up to actual insanity, and all these without any necessary accompanying paralysis.

11. That inordinate emotional expressions are often associated with the mental weakness caused by syphilis.

12. That care must be taken to distinguish certain symptoms caused by gout, from the same symptoms owing their origin to syphilis.

13. That the prognosis is better as a rule for nervous symptoms caused by syphilis than for the same symptoms depending on a lesion equal in extent, caused by another malady of the nervous centres; but that, after the arrest of the

[1] Krankheiten des Auges, Abth. III. p. 117.

disease, an indelible impression is often left upon the nerve-tissue, which manifests itself by impaired function, and which treatment cannot overcome.

14. That the iodide of potassium pushed rapidly to toleration, unless the symptoms subside before that point is reached, is the main outline of treatment. That mercury, used at the same time, or alternated with the iodide of potassium, is often of great value in protracted or inveterate cases; and that tonics, change of air and surroundings, frequently influence the effect of treatment in a marked degree, and may become essentials to success.

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Simultaneous Intra and Extra-Uterine Pregnancy.—Dr. ABRAM SAGER reports (*Michigan Univ. Med. Journ.*, Oct. 1870) a remarkable and rare case of this which presented a complication of unusual anomalies. "1st. A twin pregnancy with uterine and tubal location, the tube occupied being the *left* one. 2d. The *right* ovary containing two recent corpora lutea near the uterine end. 3d. The *right* Fallopian tube rendered incapable of functioning by a bridle across its pavilion. This form of extra and intra-uterine gestation, proceeding simultaneously, is so rare as to be scarcely recognized by most recent authorities."

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Case of Extra and Intra-Uterine Fœtation occurring conjointly; with Operation therefor, resulting in the Death of the Mother, and the Saving of Two Living Children.—This is the title of a case imperfectly reported (*New Orleans Journ. of Med.*, Oct. 1870) by Dr. E. PAUL SALE, of Aberdeen, Miss. The reporter was called in consultation (March 2d) to see with Dr. J. W. Moore a negress, æt. 22, unmarried, who had a large tumour in the abdomen. Upon examination it was concluded, he says, to be a case of extra-uterine fœtation, and in discussing the means for her relief the following points were considered. The patient "had had contractile pains for at least four weeks. She is becoming rapidly exhausted; and if nothing is done, death must soon take place. There is no chance for the fœtus to be resolved into pus or a calcareous mass. The contraction cannot be controlled, and consequently there is danger of rupture and internal hemorrhage. Ovariotomy affords the only hope for saving either the mother or child." Accordingly the next day the operation was performed by Dr. Sale in the following manner: "After cutting through the abdominal walls by successive incisions, I came upon a hard and somewhat glistening tumour. I then attempted to determine whether it was intimately connected to the uterus, by an assistant moving it up and down while my finger was in the vagina upon the os. It moved slightly, but not enough to consider it directly attached. Next as to whether it was ovarian or tubal; and I must confess the emergency of the moment precluded my making an extended examination. Since thinking of it though, my impression is that it was ovarian. I desired to cautiously incise the tumour, but the envelope was so thin and tense, that upon the first cut of the scalpel a gush of blood escaped, succeeded by a part of a sarcous mass which was soon discovered to be a placenta. The opening was immediately enlarged, and the placenta, with a living child, extracted. The sac rapidly reduced itself in size, and allowed the uterus, which it had almost entirely overlain, to be felt. To our great astonishment it was found to be large and globular as if impregnated. I stated to Dr. Moore my suspicions about it, and a hasty consultation was held whether to suspend the operation and try to deliver the child *per vaginam* by dilating the os, or make one operation do for both. The latter was deemed advisable, and hysterotomy was performed, which resulted in the extraction of another living child and placenta. The abdomen, cyst, and uterus were cleansed of all coagula, and the wound closed. A sound was passed with some difficulty through the os to allow the discharges to pass through. During the manipulation, the hands were from time to time dipped in artificial serum, and all precautions were observed as best we could under the circumstances." The patient reacted well afterwards, and she expressed herself as feeling well, but she died four days afterwards "of supposed septicæmia." No post-mortem was allowed. It is but justice to Dr. S. to say that he expresses his regret that "much more complete observations were not made; but the emergency of the moment, lack of competent assistants, lack of surgical conveniences, and entire inexperience in abdominal sections, are

excuses I offer in extenuation of this seeming remissness; and I will also state that the patient, as well as ourselves, lived in the country, and the subject of the operation lived three miles from Dr. Moore's, and ten from myself, over almost impassable prairie roads during the winter and spring months: and it so happened at the time of the occurrence, both of us were actively engaged in professional duties."

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Influence of Excessive and Prolonged Muscular Exercise upon the Elimination of Effete Matters by the Kidneys.—Prof. Austin Flint, Jr., reports (*New York Medical Journal*, Oct. 1870), some carefully conducted researches on this subject, based on an analysis of the urine passed by Mr. Weston while walking one hundred miles in twenty-one hours and thirty-nine minutes. He considers it to be established by this investigation "that excessively severe and prolonged muscular exertion increases immensely the amount of nitrogenized excrementitious matters in the urine, particularly the urea, and produces a corresponding increase in the elimination of most of the inorganic salts."

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Destruction of Tendo Achillis without much Lameness Resulting.—Dr. Hunter McGuire states (*Medical Times*, Oct. 1, 1870) that his attention had been attracted several times by the slight lameness following complete destruction of the tendo Achillis, which is so contrary to the teachings of many authors. "Moreau even goes so far as to state that 'it is better to amputate the foot if the tendo Achillis is to be destroyed.' I have lost the notes of several cases of this kind which I observed during the war, but I will mention the two following, as my recollection of them is very distinct.

"The first was a Confederate major-general, shot at McDowell, May 8, 1862. The ball entered the outside of the leg, fractured the fibula about its lower third, and passed out upon the opposite side of the limb. I enlarged the wound of entrance and removed several detached fragments of bone. Phlegmonous erysipelas attacked his leg soon after the injury, and resulted in extensive sloughing. The tendo Achillis died, was separated by the process of the disease, and was removed with the forceps. This gentleman recovered after a tedious illness, and although he has a slight limp it is so trifling that it is difficult to tell from his gait the wounded from the sound leg.

"The second case was that of a soldier wounded at Winchester by a fragment of shell. The missile tore out between three and four inches of the tendo Achillis, with the soft parts covering it. After some months the man walked with a very slight limp.

"Whether, after such a loss as I have described in the two foregoing cases, nature supplies some adventitious tissue which takes the place of the tendon; or distributes the force of the contraction of the soleus and gastrocnemius upon the tendons of the peroneal and tibial muscles; or gives to the last-named muscles increased size and strength; or whether two or all of these circumstances are combined to enable the patient to raise the heel in walking, I do not pretend to say."

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Monstrosity.—The Cincinnati Medical Reporter (December, 1870) contains a very imperfect account of a human monster brought into the world on the 13th October last, at Bennington township, Morrow County, Ohio. Some other journals, and also the daily papers, have published accounts of this monstrosity, which is pronounced by all to be unique and the most extraordinary ever seen. We have also been favoured with a description of this malformation, and with a rough drawing, both of which are too imperfect for publication, but sufficient to enable us to determine with certainty the character of this monstrosity, which is very far from being so extraordinary as is supposed.

This malformation belongs to the second family of double Monomphalien monsters of M. Isidore Geoffroy Saint Hilaire, and to his genus *Ischiopage*[1]—a genus first established by M. Debreuil under the name of Ischiadelphe.

[1] Histoire générale et particulière des Anomalies de l'Organisation chez l'Homme et les Animaux, etc.

This genus, M. G. Saint Hilaire says, "is characterized by the pelvic union of two individuals, having a common umbilicus, united together; both faces in the same direction. An ischiopage is then a double being, of very elongated form, terminated at each of its extremities by a thorax, two upper limbs, a neck, a head, and in its middle portion having a double abdomen, double lower limbs, and at its centre an umbilicus."

M. Saint Hilaire further says: "The little rarity of ischiopages, and the interest which their complexity and apparently marvellous organization have inspired anatomists with, have elicited for science numerous useful observations on the structure of these monsters, among the most important of which are those of Palfyn, of Duverney, of Prochaska, of a committee of the Medico-Chirurgical Society of Cadiz, of M. Debreuil, and of my father."

From these valuable materials, and "many other notices and articles of less interest," and also from observations made by himself on *three* ischiopages, M. Saint Hilaire gives a complete history of this remarkable genus, describing first the essential modifications of its organization, and afterwards the different complications sometimes added. For these details we must refer to the work cited.

M. Saint Hilaire has given in his work, pl. xx. fig. 1, a representation of one of these ischiopages, which corresponds with the sketch sent to us by our correspondent. Figures of this malformation are likewise given by Aldrovandus, Palfyn, Licetus, Duverney, Prochaska, Bruckmann, Hasenest, Brisbarre et Duvollier, Torlese, etc.

We will merely add, that in the Ohio case, as we are informed, both children were females, and that they weighed at birth, which was at full term, ten pounds. The mother had had three children previously; the present labour was short—only twenty-five minutes—but she says it was the most painful of her labours.

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Breech-Pin of a Shot-gun blown four and a half inches into the Brain; Recovery.—Dr. D. D. HALSTEAD, of Laclede, Mo., records (*St. Louis Medical and Surgical Journal*, March, 1870) a remarkable case of this. The subject was a healthy, temperate man, æt. 17, the breech-pin of whose gun (a single-barrelled one) was blown out when he was firing, "the sharp point of the back end striking the forehead in the frontal suture, midway between the coronal suture and the base of the os frontis, passing through the integuments and the os frontis, lacerating the falx cerebelli, penetrating the anterior lobes of the cerebrum four and a half inches, literally crushing the os frontis for a space of one and a fourth by one and three-fourths inches square, destroying one and three-fourths inches of the longitudinal sinus, and detaching the internal lamina of the os frontis from the lower edge of the opening made by the entrance of the breech-pin all the way to its base, causing severe hemorrhage from the longitudinal and frontal sinuses. A splinter of the gun-stock struck the right superciliary arch, directly above the right canthus internus, depressing that portion of the frontal bone immediately superior to the nasal bones, so far as to cause its fracture from the internal canthus of the left orbit upwards into the main aperture made by the breech-pin.

"When injured he was alone. His own statement is that when injured he did not for a moment lose consciousness, but the shock was so severe that he dropped on his hands and knees, and for some time heard a loud, prolonged ringing in his ears. After a few moments he rallied from the shock, and in wiping the blood from his face, he felt the breech-pin protruding about half an inch from the forehead. This he got hold of and pulled out. He then washed his face in a pond of water close by, and lay down a short time to rest himself, after which he got to his horse, and, mounting, rode to the nearest house, over a mile distant, where he arrived bewildered and half unconscious. When I first saw the patient, nearly twelve hours after the accident, I found him lying on a bed in a semi-comatose condition, the eyes swelled and shut, the upper part of the face and forehead burnt and blackened with the powder. Dr. Thomson had been called in, and had sewed the wounds in the scalp as well as he could, and the brain broken up was finding its way freely to the surface between the sutures. Considerable hemorrhage was taking place through the posterior

nares into the stomach, from which the blood was ejected every few minutes. The house in which I found him being a log cabin, very open and without a window or any other means of obtaining light, except through the door or between the logs where the chinking was out, I had him placed on a sleigh and removed to his own home, nearly three miles distant. I then proceeded to operate, making an incision through the integuments in the shape of an inverted U, commencing a little inside of the centre of the left superciliary arch, passing upward, to the right, and downward, terminating at a corresponding point over the right superciliary arch, including both the wounds within the line of the incision. On separating the integuments from the cranium within the circle of the incision, a large opening in the os frontis, measuring one and three-fourths inches in length, extending from the centre of the frontal bone downward towards its base, including the frontal suture, and one and a quarter inches wide, extending from the right side of the frontal suture to the left. The opening was filled up with the protruding substance of the broken brain, intermingled with fragments of bone. The latter I removed, and then inserted my finger in the wound its whole length; I could not reach the termination of the track made by the breech-pin, but was enabled to ascertain the depth and locality of numerous fragments of bone, some of which were over two inches from the surface. These I extracted as carefully as I could; also a three-fourth inch screw, which was buried about two inches in the substance of the brain, making in all over thirty-eight fragments of bone removed from the wound, and one three-fourth inch screw. Some of the largest fragments were over half an inch square. The direction of the iron from its place of entrance was backward toward the occiput; slightly downward and to the right, apparently nearly touching the petrous portion of the right temporal bone. Passing my finger downward along the frontal suture inside the frontal bone, I ascertained that the internal plate was separated from the external all the way to the base of the os frontis, leaving the frontal sinus destitute of its posterior wall, and its anterior wall completely detached from the os frontis and nasal bones, but attached to the integuments. This I placed in proper position as well as possible, and placing the integuments in position over the fracture, secured with sutures, and ordered cold water kept constantly applied to the whole anterior portion of the head, and gtt. x of conct. tinct. of gelsemium given every two hours, and in event of uneasiness or severe pains, morph. sulph., gr. one-sixth, to be given every four hours."

The accident occurred October 21, 1869; we omit the diary of the case, and need only add that the report on the following January 10th is as follows: "Rode to Laclede to-day, a distance of five miles, on horseback; tells me he has been at work for several days; is very fleshy and ruddy-looking; sinus still open, but discharging very slightly; four or five small spicula of bone have worked out since I last saw him, on the 3d ult.; says he experiences nothing unusual or different in his condition from what it was before he was injured; recollects perfectly everything that transpired previous to his removal home, but has no recollection from that time until the 30th of October; his statement of his actions subsequent to the injury was fully confirmed by the appearance of the ground where he had wandered, leaving a pool of blood to mark each place where he had halted a moment to rest or to wipe the blood from his eyes."

A New Method of Delivering the Aftercoming Head in Contracted Pelvis.—Dr. Wm. Goodell, in a communication to the Philadelphia Obstetrical Society (Sept. 1, 1870), gives the following account of this method which he has successfully employed:—

In a brim, contracted in its antero-posterior diameter, the sacral side of the after-coming head becomes fixed on the point of the projecting promontory, and its extrication can take place only when the pubic side of the head can be made to revolve around the promontory as a centre of motion, by descending over the smooth surface of the symphysis. And this can the more readily be done in such cases, because the sacrum is so scooped out and so shortened, that traction can be often made in a line posterior to the axis of the superior strait. Bearing this fact in mind, after grasping the neck of the child with one hand

and the ankles with the other, the physician should make his first movement of traction in the axis of the *outlet*—indeed in a line anterior to this axis—by, at the same time, forcing the child's neck well against the pubes—for then the pubic side of the head will recede from the inlet, whilst the sacral side will proportionately descend over the point of the promontory, and affront the inlet. This change of position can be materially aided by an intelligent assistant, who will make pressure with both hands, through the flaccid abdominal walls, upon the vault of the child's head from before, backwards and downwards. By this manœuvre the promontory will nip the head at a point higher up, and nearer to its vault; hence, the arm of the lever, measured by a line drawn from the base of the skull to the point nipped by the promontory, will be correspondingly lengthened—an advantage not to be overlooked. If now, *without for a moment relaxing the original traction-force*, its direction be reversed, and the body of the child be swept backwards upon the coccyx—the neck being also forced downwards and backwards into the hollow of the sacrum—and if the assistant at the same time reverses the direction of the external pressure upon the vault of the head, making it now from behind forwards and downwards, the pubic side of the child's head is made to revolve around the promontory and descend with the least expenditure of traction-force, and consequently with a much greater chance of safety to the child. Of course, as soon as the head has passed the brim, the line of traction must be changed to that of Carus' curve.

Dr. G. stated that, by this method, he had, thus far, delivered three women with unusually bad pelves; in one of them the child's head had to be lessened in a previous labour. His last case presented so many points of interest, he would briefly relate it. A secundipara, in her previous labour, had fortunately been prematurely delivered at the end of the seventh month of gestation. The child was still; weighed under five pounds, and yet her labour lasted over forty-eight hours, six of them being attended with extreme suffering and violent expulsive pains. On the present occasion, she went to term against the wishes of her attending physician, who had urged the induction of premature labour. At 8 A. M., when labour had lasted ten hours, Dr. G. found the water dribbling away from an expanded os uteri. The head, presenting the occiput to the left ilium, could not bear upon the cervix, but rolled about upon the shelf formed by the promontory. By careful measurement with the finger the conjugate diameter of the brim was estimated at a trifle over three inches. It was his intention to dilate the os with the water-bags and apply the forceps. But, while he was preparing to do so, the woman suddenly began to twitch her muscles, start convulsively, and to complain of blindness and intense headache. Fearing an attack of eclampsia he determined to deliver at once. The woman was accordingly thoroughly etherized, and brought down to the edge of the bed in the dorsal decubitus. Two fingers of the left hand were then squeezed into the os, and ultimately a third; by which, in conjunction with the right hand externally, version was made with great ease. One foot having been drawn down, a short time was allowed to elapse before the body and arms were delivered, in order that the breech might dilate the os. Then, by following the method he had just described, the child was soon delivered in an asphyxiated condition, but promptly recovered. It presented a deep indentation on the right side of its head, weighed seven pounds and twelve ounces, and next day measured three and a half inches at the bi-parietal diameter.

In conclusion, Dr. G. remarked, that so much power and time were gained in ordinary pelvic presentations by the plan of simply pushing the head through the brim,—a plan which he had first learned from Prof. R. A. F. Penrose,—that he was surprised it was not oftener resorted to. For, although the neck of the child could bear a great strain without breaking, it was desirable to diminish the chances of this risk, as well as to deliver as soon as possible; since few children survived a delay at the brim of over five minutes' duration.—*American Journal of Obstetrics*, Nov. 1870.

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At the Annual Meeting of the Committee, it was voted that Prizes of Two Hundred Dollars each, be awarded to Henry P. Walcott, M. D., of Cambridge, Mass., and to G. D. Daly, M. D., of Lesueur, Minnesota, for dissertations on "Aphasia, or the Relation of the Brain to Speech."

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